ON THE

NATURE AND TREATMENT

OF

DIABETES.

RESEARCHES

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OF

DIABETES.

BY

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TO

THOMAS TURNER, ESQ., F. R. A. S.,

TREASURER OF GUY'S HOSPITAL,

THIS VOLUME

Is respectfully Dedicated

IN WARM APPRECIATION

OF

THE HIGH-MINDED AND LIBERAL INTEREST

HE HAS TAKEN IN

THE WELFARE OF THE MEDICAL PROFESSION,

AND

IN GRATEFUL ACKNOWLEDGMENT

OF THE

ENCOURAGEMENT HE HAS ALWAYS SO WILLINGLY BESTOWED TOWARDS

THE PROSECUTION OF SCIENTIFIC RESEARCH.

PREFACE.

It is now, for upwards of nine years, that a great portion of my time has been specially devoted to the subject comprised by the present volume. Working as I had done, whilst in Paris, under the originator of the glycogenic theory, it was not unnatural that I should have been impressed with a desire, on my return to England, to prosecute a continuance of his researches, in a direction where it was admitted a deficiency existed.

Taking it for granted, that the views I had learnt, and which had obtained such universal acceptance, were substantial and true; I endeavoured, at the onset of my researches, to make out the nature of the process by which sugar underwent its supposed destruction in the lungs—the only point upon which, it seemed, there really remained anything connected with the subject to be disclosed.

From the early results I obtained, I formed an opinion regarding sugar-destruction which, however, subsequent research proved to me was untenable. I had not, at first, a shadow of reason to induce me to imagine that the doctrine I was proceeding on, was otherwise than unimpeachably correct. Time, however, led me to discover, that in my starting-point I was resting upon a fallacious foundation. Time revealed to me, that the mode of experimenting which

had been hitherto adopted, had conducted to the formation of erroneous conclusions, regarding the main points relied upon, in support of glycogenesis. The discovery of truth, being the only admissible goal of scientific research, I could not hesitate, however much I might expose myself to the criticism of others, to abandon the support I had previously given to the glycogenic theory.

The facts I discovered, formed the subject of a communication, published in the 'Philosophical Transactions' for 1860; they are fully brought forward in the physiological part of this work. No one, I think, will deny that these facts materially alter the position that was formerly maintained. To my mind, they refute the validity of the doctrine of glycogenesis, and I feel myself justified in saying, that time has only served to strengthen all that I originally stated. Whatever fresh evidence I have obtained has given me support; and, nothing that I have seen, which has been adduced in opposition, would appear to me to call for the slightest alteration.

The points that are here referred to, present, I believe, a bearing of importance in reference to the pathology of Diabetes. Latterly, ideas have been fixed upon an excessive production of sugar in the liver, on the one hand; or, a defective destruction in the lungs, on the other; to account for the elimination that takes place in the complaint. But, the results that have been now obtained, looked at in the light I read them, carry us back to the position maintained by our countryman Dr. Prout previous to the promulgation of the glycogenic theory.

Although I am obliged, then, to dissent from Bernard's

glycogenic views, yet, I most willingly admit, how much this subject is indebted to him for the present advanced position it may be considered to hold. To this distinguished physiologist, likewise belongs the merit of an unquestionably important discovery; for, it was he, who first made known the amyloid substance of the liver.

Acting upon the principle of avoiding the ingestion of starch and sugar in the treatment of Diabetes, I have instituted a substitute for bread out of almonds and eggs. This will be found referred to, in the latter portion of the volume, and I eagerly hope it may prove a desirable accession to the limited list of articles from the vegetable kingdom, that, under such a restriction, the Diabetic is allowed to consume.

33, Bedford Place, Russell Square;
May 10th, 1862.

Fig. 1.

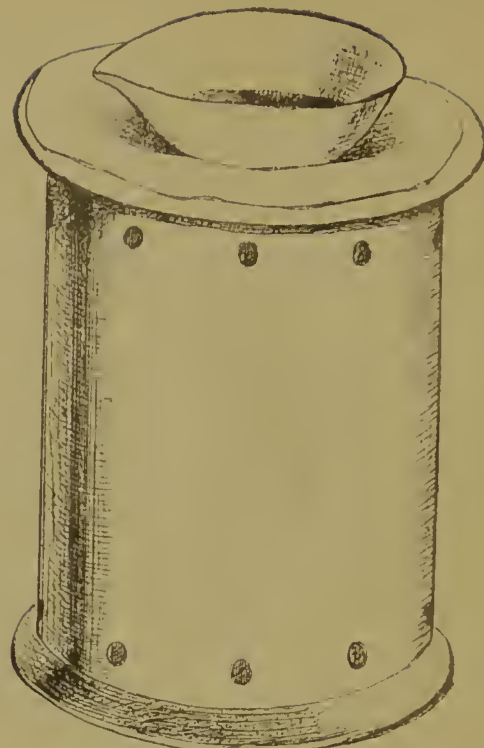


Fig. 4.

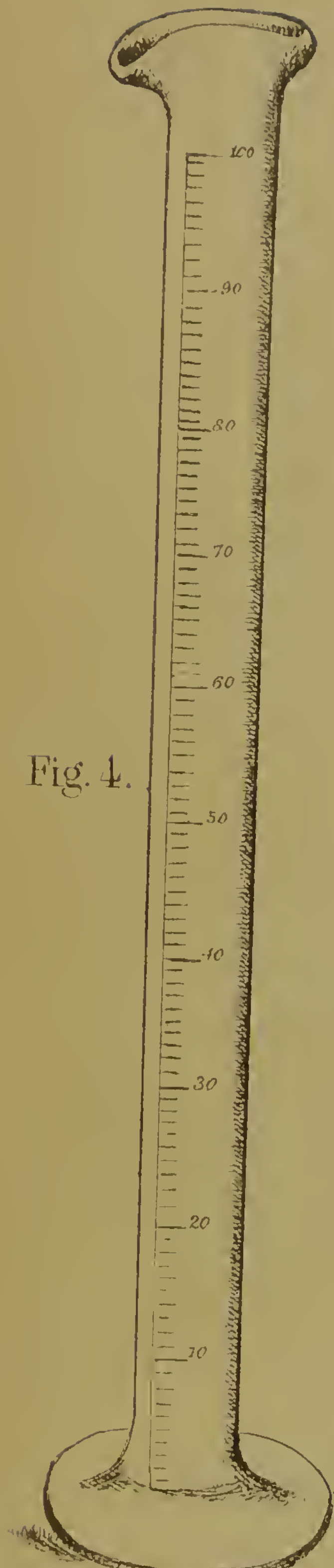
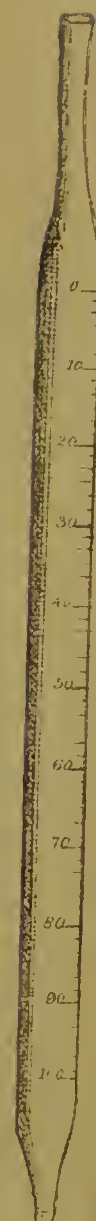


Fig. 2.

Fig. 3.

Fig. 5



RESEARCHES
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PART I.

On the Detection of Sugar. Qualitative and Quantitative Analysis.

Of the varieties of sugar, glucose or grape sugar is that which is encountered in investigations upon the subject of diabetes. As a preliminary step, it is of the first importance that the means adopted for recognising our agent should be agreed upon as satisfactory. Happily, we do not here, at the onset, encounter any difficulty. Such strikingly characteristic, and easily recognisable properties are possessed by glucose, that it is almost as readily susceptible of detection as any inorganic material; and this, even, when present to an exceedingly minute extent. Numerous tests have been from time to time recommended, but, it is only to those, that have been employed in my investigations, that I consider it necessary in the following pages to refer. These are the alkali, the cupro-potassic, and the fermentation tests.

The alkali, or Moore's test, consists in treating the specimen to be examined with a solution of potash, and then boiling. The alkali decomposes the sugar, resolving it into a dark-coloured substance, which gives to the liquid more or less of a sherry or brownish hue. I do not consider this test a desirable one for physiological purposes. Where the quantity of sugar is large, as in diabetic urine, the effect is so strongly marked that there cannot be any mistake about it; but, where the quantity of sugar is small, there is only a slight difference of colour to appreciate. Now, it happens, even with the best-intentioned efforts, that we are all apt to be deceived, where shades of difference, particularly in colour, are relied on. What, certainly for physiological purposes, we require is something giving a more decided "aye" or "nay," and this we get with the copper and fermentation tests.

In using Moore's test as a means of detecting diabetes, a drachm, say, of urine is treated in a test-tube, with about half its bulk of liquor potassæ, and boiled for a couple of minutes over a spirit lamp. It must be borne in mind that a fallacy—and I have frequently seen it—may occur. If the liquor potassæ have been kept in a white or flint-glass bottle, it becomes contaminated with lead. Lead may, also, have been abstracted from the glaze of an earthenware dish, in which the potash may have been boiled or evaporated down. At all events, from whatever source the lead has been derived, on boiling the liquor potassæ contaminated with it, with any organic compound containing sulphur, the sulphur is liberated, and, combining with the metal, produces a more or less dark-coloured liquid, according to the amount of sulphuret formed.

For reasons that will thus be apparent, it may be inferred that I rarely resort to the use of the liquor potassæ test. It is only, indeed, in the examination of urine that

I ever employ it; and, then, only when I do not happen to have the copper test at hand.

The copper test is certainly, according to my own experience, by far the most useful test that we have for sugar. There are various forms of it; but in all, the principle of action is the same. Free protoxide of copper is present, which, on being boiled with grape-sugar, loses half its oxygen, and is thrown down as a yellow, orange-yellow, or orange-red precipitate, according to the amount of sugar present, or the degree of concentration, which possibly determines an alteration in the state of hydration of the deposit. A liquid containing a large proportion of sugar produces an orange-red deposit; whilst one containing a slight proportion only, gives rise to a yellow-coloured deposit.

In Trommer's test, the oxide of copper is set free at the time the test is employed. A drop, or a couple of drops, of a moderately strong solution of the sulphate of copper, being allowed to fall into the specimen to be tested, liquor potassæ is then added in considerable excess. The precipitate, at first thrown down on the addition of the alkali, is re-dissolved when sugar is present, and a deep blue coloured liquid is the result, which, on boiling, deposits the reduced or sub-oxide precipitate. Trommer's, is, I consider, less serviceable than the other forms of copper test, on account of its being less convenient and less ready of application; and, from the fact that where only traces of sugar are present, they may escape detection, in consequence of undissolved protoxide obscuring a slight production of reduced oxide that may have taken place.

In the other copper tests, the oxide of copper, thrown down by the alkali, is held in solution by the presence of some organic material, which does not occasion its reduction at the temperature of ebullition. It is the property of the

protoxide of copper not to be soluble in an alkali unless some form of organic matter is present, when perfect solubility is the result. In these solutions it is, of course, necessary to employ some form of organic matter that does not possess the power of deoxidizing the metallic oxide at a boiling heat. The tartaric acid is of this kind, and is the agent commonly selected for introduction into the test. Barreswil's liquid, which is the form of copper test that has been employed by Bernard in his researches, is made with the cream of tartar. Reduced to our English scale of weight and measurement, it is thus composed:

Barreswil's Test Solution for Sugar.

Bitartrate of potash (cream of tartar)	960 grains.
Carbonate of soda (crystallized) .	960 ,,
Caustic potash (potassa fusa) . .	640 ,,
Sulphate of copper	320 ,,
Distilled water	20 fluid oz.

These materials are to be dissolved and mixed, and the resulting blue liquid to be filtered.

The use of the bitartrate of potash in the above liquid calls for the employment of the carbonate of soda to neutralize its excess of acid. It appears to me more simple to take at once the neutral tartrate of potash, and then the carbonated alkali may be dispensed with. Fehling's solution is of this description, and consists of sulphate of copper and tartrate of potash, with soda as the alkali.

The test that I employ in my laboratory is a modification of Fehling's solution, containing caustic potash instead of soda. The following are the proportions of the ingredients taken. Used quantitatively, the strength is such, that half

a grain of grape sugar exactly reduces the oxide contained in 100 minims of the solution.

Modification of Fehling's Test Solution for Sugar.

Sulphate of copper . . .	320 grains.
Tartrate of potash (neutral) . .	640 ,,
Caustic potash (potassa fusa) .	1280 ,,
Distilled water	20 fluid oz.

Dissolve the tartrate of potash and caustic potash together in one portion of the water, and the sulphate of copper alone in the other; afterwards mix.

Where a test solution is not required in sufficient quantity to lead to its being kept ready prepared, I can recommend the following as a simple method of extemporaneously preparing one for use. Take five grains of sulphate of copper and ten grains of tartrate of potash (neutral), and dissolve in two drachms of liquor potassæ. A clear deep-blue liquid is formed, which is quite as efficient as any other kind of cupro-potassic test.

There is a circumstance connected with the use of all the copper solutions, that it is exceedingly important to be aware of. If the test liquid be kept for any considerable period, and particularly if exposed to light, it will of itself deposit some red oxide on boiling. Probably, an action occurs by which the free is converted into the carbonated alkali. Hence, unless the solution have been recently prepared, it should be tested, by boiling a little, alone, from time to time; and if it should be found to give a precipitate, a fresh addition of potash will completely renovate it—will render it, in fact, again as fit as ever for use.

Although the test may be good, yet the precipitation of sub-oxide from a copper solution must not be looked

upon as affording an infallible indication of the presence of sugar; neither, on the other hand, must the absence of a precipitate be regarded as unequivocally proving the absence of sugar. Such a remark must seem materially to detract from the value of this test; but, bearing in mind what has been mentioned above, and other considerations to follow, I do not think we are much exposed to fall into error. At all events, the other tests are not without their objections, and my experience decidedly is, that the copper, is the most useful test we have.

I have seen it remarked, that glycerine, tannine, cellulose, leucine, uric acid, and chloroform, are each capable of producing, in different degrees, a reduction of the oxide of copper. Chloroform certainly exerts a strong reducing effect. Uric acid I have witnessed occasion some deposit of red oxide; and from cotton I have obtained just a trace of precipitate. But the glycerine I have tested has not yielded any reaction.

There are circumstances under which no deposit of sub-oxide may be observed, although sugar may be indisputably present. Ammoniacal salts have the property of giving rise to this occurrence. I have frequently noticed, where, an ammoniacal odour has been evolved on boiling a specimen of urine, for example, in diabetes artificially induced in animals, with the blue liquid, that at first there has been no perceptible change; then, a change of colour, without any precipitation has taken place; and, when the boiling has been continued, so that the potash of the blue liquid has expelled the whole of the ammonia, a considerable deposit of sub-oxide has fallen.

The presence of albumen interferes with the proper reaction of the copper test. I expect this depends upon the ammonia evolved as the result of the destruction of the albumen by the potash in the test. In a case of apoplexy,

where the urine was albuminous, I tested for sugar, expecting to find it, because there was sudden and great congestion of the circulation. The action of the test was obscure, indeed, I could not say that it afforded a decided indication of the presence of sugar, until after the urine had been previously boiled and filtered, to get rid of the albumen, when a neat precipitation of sub-oxide occurred.

I have noticed, also, in testing prepared liquids during my physiological researches; that, an incomplete separation of albumen has concealed the presence of sugar, when existing only to a slight extent. It is, then, particularly necessary always to secure that exposure to fallacy from such a source should be avoided.

The urine, under ordinary circumstances, requires no preparation for the application of the copper solution, even to display the presence of a slight quantity of sugar. Where, however, the existence of but traces only of sugar are sought after, the urine may be concentrated by evaporation, and treated with the acetate of lead to get rid of colouring matter, urates, and phosphates.

My researches have not been of a nature to have given me much experience upon the detection of very minute quantities of sugar in urine. Dr. Bence Jones, however, has made an able analysis of the relative merits of the principal processes that have been suggested, and has furnished his results in a paper published in the 'Quarterly Journal of the Chemical Society' for April, 1861. From one of Professor Brücke's processes the best results were obtained. One seventh of a grain of sugar, dissolved in 200 cubic centimètres of urine, could be detected, and two thirds of all the sugar added, was recovered. The process consists in precipitating the urine with neutral acetate of lead, and then with basic acetate of lead; and, after filtering off the precipitate,

adding ammonia. In this last precipitate, the chief part of the sugar present will be found. The ammonia precipitate is to be treated with a solution of oxalic acid, or, what is better, according to Dr. Bence Jones, with sulphuretted hydrogen, to separate the lead. The filtrate is colourless, and contains the sugar in a state fit for detection by any of the sugar tests.

With a fluid like blood, highly coloured and albuminous, as such a fluid is, a process of preparation is absolutely indispensable before, under any circumstances, our copper test can be applied. But, exposure to boiling is not sufficient to effect a complete precipitation of the albumen and colouring matter, on account of the existence of the alkalinity which belongs to blood. The cautious addition of acetic acid to the point of neutralization, in conjunction with the boiling, will lead to the object that is desired, and furnish a limpid, colourless fluid; but an excess, even a very slight excess of acid, operates in the same way as the original alkalinity. Being a solvent of albuminous matters, a slight excess of acetic acid exactly produces the condition that its employment was intended to remove.

A much less troublesome process, for preparing a liquid like blood, to test, is by boiling it with some sulphate of soda. A small quantity of the fluid—say half an ounce—is placed in a small porcelain capsule, and into it is thrown about its own weight of sulphate of soda, in crystals. On briskly boiling, a complete separation of albumen and colouring matter is effected, so that, when thrown on a filter, a limpid, transparent liquid immediately runs through. The only precaution needed, is to add enough of the salt to ensure a total separation of all that is coagulable. The presence of the sulphate of soda in the filtered liquid does not in the slightest degree interfere with the application of the copper test.

For a solid organ, such as the liver, the method of procedure to prepare for testing, may be; either, to make a plain decoction of it, or else to pound it in a mortar with nearly an equal bulk of the sulphate of soda, and then heat and filter in the same manner as with blood.

Animal charcoal has been more lately recommended for separating sugar from some other materials. The absorbent power of animal charcoal is a well-known property; but, according to Bernard,* glucose is a substance that it does not take up. Hence (still following the same authority), although it removes albumen and colouring matter from blood, albumen and uric acid from urine, and even caseine and fatty matter from milk, yet, sugar present in either case will be left and pass with the liquid through the filter. I have several times tried this process upon blood, but have no hesitation in giving a decided preference to the old plan with the sulphate of soda.

Although a point of doubt might exist in the minds of some concerning the reliance to be placed upon the indications afforded by the tests I have mentioned, yet the fermentation test has been hitherto generally looked upon as perfectly characterising by its reaction the presence of sugar. It seems, however, from the researches of a French chemist, M. Berthelot, who has recently devoted much attention to the chemistry of the sugars, and whose statements are entitled to the greatest consideration, that there are other substances besides sugar—viz., glycerine, mannite, dulcine, and sorbine, which are capable of undergoing the alcoholic fermentation with yeast. Alcoholic fermentation, therefore, can no longer be regarded as affording an infallible indication of sugar.

For the application of the fermentation test the process is exceedingly simple. A contrivance such as is repre-

* 'Leçons de Physiologie, Cours du semestre d'hiver 1854-55,' p. 45.

sented at Plate, fig. 3, will be found the most convenient. The urine or liquid to be examined is mixed with yeast and placed in the test tube, which ought to be filled to the top. The bent tube is then introduced, and the cork tightly adapted. The end of the bent tube should nearly reach the bottom of the test tube, so that as the gas, resulting from fermentation, is generated and rises in the test tube, it may cause, by its pressure, an expulsion of the liquid, which is to be received into a glass suitably placed for the purpose of collecting it. The test tube is immersed in a vessel of tepid water, and placed in a warm situation, as before a fire; or else, the water should be renewed as occasion may require. By this application of warmth, when grape sugar is present, fermentation in a few minutes commences briskly to take place, and the generated gas, rising in the test tube, displaces the liquid, which becomes transferred to the glass.

For ordinary purposes the evolution of gas may be taken as a proof of fermentation; but, should it be considered to require confirmation, the actual production of alcohol and carbonic acid may, in the following manner, be easily established.

The test tube, which has been gradually emptied of its contents and is supposed to be filled with carbonic acid gas, is inverted and the cork removed under water. A small fragment of potash being introduced, the thumb is applied to the extremity to close it, and agitation freely performed for a minute. The carbonic acid is absorbed by the potash and a vacuum formed, which is shown by again inverting the test tube, and, with its mouth under water, removing the thumb. The water instantly ascends and occupies the space of the carbonic acid that has been absorbed.

To recognise the alcohol, the liquid which has been dis-

placed from the test tube is still kept in a warm situation, to favour, by the completion of fermentation, the production of as much of it as possible. The liquid is then submitted to distillation, and, when about a third of its bulk has passed over, this is mixed with caustic lime and placed in a test tube provided, as represented at Plate, fig. 2, with a cork and small glass tube drawn out at one end to a fine-pointed extremity. The lime combining with the water, the alcohol is expelled in a tolerably pure form on the application of heat. It may be set light to, and will be found to burn with a pale-blue flame, as it issues through the pointed extremity of the tube.

The presence of alcohol may also be displayed by the bichromate of potash and sulphuric acid test. This process may be employed when the alcohol is in too small a quantity to be detected by ignition. The fermented product is submitted to distillation. The first few drops that pass over are collected and poured into the test, consisting of a moderately strong solution of bichromate of potash treated with a small quantity of sulphuric acid. On being gently heated, the fluid becomes of a beautiful emerald green, should alcohol be present.

In applying the fermentation test, it is necessary that the yeast should be thoroughly washed before use. Yeast, as purchased, will undergo a considerable fermentation alone, and even after being well washed, I have found it give rise to the evolution of a slight amount of carbonic acid. In fact, the fermentation test in my hands has far from proved susceptible of that delicacy and absolute precision I could have desired.

The tests I have been speaking of have had reference to glucose, or grape sugar, which is the only kind of sugar the pathologist has to deal with. In conducting

physiological investigations, however, it may be necessary to seek for cane sugar, which is not sensitive to either form of the copper test. To render it so, it must first be converted into glucose; and this may be done by boiling the specimen supposed to contain cane sugar for a short time with a small quantity of sulphuric acid. The acid employed being neutralized, the copper test will indicate if any glucose have been formed.

For the quantitative determination of sugar, the process I always adopt is with the cupro-potassic solution; and this, my experience enables me to speak of as being susceptible of very great delicacy. We do not separate and weigh the sugar, as the chemist does with an inorganic material; but, on the other hand, estimate its amount by its extent of reducing or deoxidizing effect on a copper solution of standard strength. The copper solution I employ is that composed of the tartrate of potash, potash, and sulphate of copper, the precise composition of which is to be found at page 5. This liquid is of such a strength that 100 minims of it are decolorized by half a grain of grape sugar—in other words, half a grain is the exact amount of sugar required to convert the whole of the oxide of copper into the state of sub-oxide that is contained in 100 minims of the liquid. The sugar from which this determination of strength was drawn, was specially sought after to procure as pure a specimen as possible. It was obtained from Mr. Morson, and was thoroughly deprived of water by drying in a steam oven before being used.

In describing how the quantitative analysis is performed, I will give an illustration of the method of procedure I adopt in the case of a specimen of diabetic urine.

In an ordinary case of diabetes the urine is too concentrated with sugar to operate upon alone, so it is diluted with four times (this I have found about the most con-

venient extent of dilution) its bulk of water. For the process of dilution, I use a tall, narrow, graduated glass, such as is represented at Plate, fig. 4. It is divided into 100 measures; and I therefore fill up to 80 with distilled water, and then to 100 with the urine. The mixture is emptied into a vessel, and poured backwards and forwards once or twice, so that the two may become thoroughly mixed together.

One hundred minims of blue liquid are now taken and placed in a small porçelain capsule, with a fragment of solid caustic potash about double the size of a pea. This addition of potash causes the precipitate to fall in a denser form, and thus leaves the colour of the liquid more easily seen. The capsule is placed over the flame of a spirit lamp on a retort-stand, or what is better, on a piece of iron-wire gauze, fitting on the top of an earthenware cylinder, as at Plate, fig. 1. The cylinder protects the flame of the spirit lamp from draught, and the gauze distributes the heat and renders the boiling much more steady. The liquid in the capsule is to be made to boil gently, and then, the diluted urine is dropped into it slowly from a graduated tube, until the blue colour is entirely removed. The contents of the capsule having been kept steadily boiling all the time, as soon as the blue colour has disappeared, the amount of diluted urine employed is to be read off from the graduated scale of the tube.

Usually when the operation is slowly and properly performed, the red oxide produced, collects at the bottom of the capsule, leaving the liquid above clear, the colour of which may be gradually observed to fade, and finally disappear. At other times, the reduced oxide is more or less distributed through the liquid, but even then, it is not difficult, after a little experience, to decide when the point required, is attained. All trace of blue or green should be just,

and no more than destroyed, and a pure orange or orange-red produced. It is desirable occasionally with a glass rod to turn the capsule a little on one side, so as to bring the fluid over a clean part of the surface, where no sub-oxide has fallen, in order that against a white ground any trace of remaining colour may be more easily perceived.

The tube I employ for measuring the blue liquid, and for dropping the diluted urine into the capsule, consists of a long, narrow pipette, the form of which is represented at Plate, fig. 5. It is graduated in minims; each minim, in accordance with the Pharmacopœia standard, being equivalent to the 480th part of a fluid ounce, or the 60th part of a fluid drachm. The scale contains 100 minims, and commences above; so that, starting at 0, the fluid which has been allowed to escape may, without any calculation, be read off. Tubes constructed according to the description I have given are to be procured at Mr. Griffin's, Bunhill Row, London.

In following out an example, let it be supposed that a diabetic person has passed ten pints of urine during the twenty-four hours. It has been all mixed together, and a mean specimen of the whole taken for analysis. A portion is diluted with water, to the extent of one part in five. 100 minims of the blue liquid have been measured out into a capsule, and a fragment of caustic potash introduced. The liquid having been made to boil gently, the diluted urine is dropped slowly into it from the graduated pipette, the escape being controlled by the finger placed at the top, until a decolorization is effected. Having started at 0, the amount of diluted urine required for this purpose can be read off from the graduated scale by a glance at the height of what remains in the tube. Suppose twenty-eight minims to be the quantity that has been allowed to escape; this is noted down, and the analysis is complete. It now

only requires a little calculation to ascertain the amount of sugar eliminated during the twenty-four hours.

One hundred minims of blue liquid, requiring half a grain of sugar for decolorization, and 28 minims being the amount of the diluted urine that has been taken to decolorize this quantity, gives to the 28 minims of diluted urine half a grain of sugar. By the simple rule of proportion, the amount of sugar per ounce is now ascertained. Thus—

	Minims of diluted urine.		Grain of sugar.		Minims of diluted urine.		Grains of sugar.
As	28	:	·5	: :	480 (1 fluid ounce)	:	8·57

But it is an ounce of the *diluted* urine that contains 8·57 grains of sugar, and the extent of dilution being such that the ounce is composed of only a fifth part of urine, it is this fifth part of the ounce that represents the 8·57 grains of sugar. Therefore the 8·57 must be multiplied by 5 to get the amount of sugar present in the ounce of undiluted urine, $8·57 \times 5 = 42·85$. The urine thus containing 42·85 grains of sugar per ounce, and the patient having passed 10 pints, or 200 ounces, during the twenty-four hours, gives 8570 grains as the amount of sugar eliminated for this period.

Although this process is somewhat long to describe, yet, with the requisite conveniences at hand, and with the tact acquired by a little experience, it can be quickly and easily applied. That it is susceptible of a fair—of even a minute degree of accuracy, I have proved upon repeated occasions. For instance, I have frequently taken a number of different specimens of diabetic urine, and determined the amount of sugar in each. I have then mixed all the specimens together, and ascertained by analysis the amount of sugar existing in the whole. The figures given by the amounts of the several specimens added together, have corresponded

within a very little of the figures given by the analysis of the whole. For many days, in a case of diabetes, when I directed the urine to be collected in separate vessels for every four hours, and when I had thus six specimens a day for analysis, I checked the results derived from these six analyses, by mixing all the urine together, and making an examination of a specimen of the whole. I used generally to bring the amounts given by these two processes for the twenty-four hours within about twenty or thirty grains of each other.

To avoid repeating the calculation for ascertaining the quantity of sugar per fluid ounce, from the number of minims taken to decolorize, for each particular instance, I have worked out the amounts for every minim, between 15 and 100, and framed them into a table, which I keep at hand for reference, and find of the greatest convenience. This table I introduce for the assistance of others. In using it, all that is needed, is to look opposite the number of minims required for decolorization, to find the amount of sugar contained in each fluid-ounce of the specimen submitted to analysis. Proper account must afterwards be taken of the extent of dilution that has been employed, when speaking of the sugar per ounce that the urine itself contains. To give an example, suppose 26 minims to constitute the quantity of a specimen required to decolorize 100 minims of the blue liquid, and the specimen to be composed of one measure of urine diluted with four measures of water; then the sugar per ounce in the specimen examined will be 9·23 grains, and in the undiluted urine (9·23 × 5) 46·15 grains.

Table showing the quantity of sugar per fluid ounce, for minims, from 15 *to* 100, *required to decolorise* 100 *minims of my blue liquid.*

Minims to decolorise.	Sugar per fluid ounce.	Minims to decolorise.	Sugar per fluid ounce.	Minims to decolorise.	Sugar per fluid ounce.
15	16·	44	5·45	73	3·28
16	15·	45	5·33	74	3·24
17	14·11	46	5·21	75	3·20
18	13·33	47	5·10	76	3·15
19	12·63	48	5·	77	3·11
20	12·	49	4·89	78	3·07
21	11·42	50	4·80	79	3·03
22	10·90	51	4·70	80	3·
23	10·43	52	4·61	81	2·96
24	10·	53	4·52	82	2·92
25	9·60	54	4·44	83	2·89
26	9·23	55	4·36	84	2·85
27	8·88	56	4·28	85	2·82
28	8·57	57	4·21	86	2·79
29	8·27	58	4·13	87	2·75
30	8·	59	4·06	88	2·72
31	7·74	60	4·	89	2·69
32	7·50	61	3·93	90	2·66
33	7·27	62	3·87	91	2·63
34	7·05	63	3·80	92	2·60
35	6·85	64	3·75	93	2·58
36	6·66	65	3·69	94	2·55
37	6·48	66	3·63	95	2·52
38	6·31	67	3·58	96	2·50
39	6·15	68	3·52	97	2·47
40	6·	69	3·47	98	2·44
41	5·85	70	3·42	99	2·42
42	5·71	71	3·38	100	2·40
43	5·58	72	3·33		

It is especially important in a quantitative examination for sugar, that the analysis should not be delayed many hours, on account of the decomposition that is liable to set in. During the hot weather of summer, it is not very long after diabetic urine has been passed, before active fermentation may be observed to occur. And, unless the chamber vessel is thoroughly cleansed each time it is emptied, fermentation is almost immediately

set up, by contact with the stale urine left adherent to the surface.

My experience latterly has been confined to the above process as a means of arriving at a quantitative determination of sugar. I have found it so satisfactory in its application, that I have not had occasion to employ any other. Formerly, however, upon several occasions I resorted to the fermentation test, in which an estimate of the amount of sugar is formed from the quantity of carbonic acid evolved. According to Dr. Christison, the evolution of one cubic inch of carbonic acid corresponds in round numbers to one grain of sugar; or, speaking more precisely, forty-seven cubic inches of gas are equivalent to forty-five grains of sugar.* I cannot say, from what I have seen of this test, that it is susceptible of the minute degree of accuracy that could be desired, and certainly, for facility of application, it is not to be compared to the process with the cupro-potassic solution.

* 'Library of Medicine,' vol. iv, p. 249, art. "Diabetes Mellitus."

PART II.

On the Physiological Relations of Sugar.

Diabetes mellitus consisting of an unusual flow of sugar from the body with the urine, the first step to be taken in an investigation of the nature of the disease, is to establish clearly, the natural or physiological relations of sugar in the animal economy. It will be readily conceded, I think, that a knowledge concerning the abnormal bearings of sugar in the economy, is not likely to rest upon any satisfactory footing, until a correct notion has been obtained of the state that is normal. It is only, I conceive, through physiology that we are in a position to approach advantageously the pathology of diabetes.

Willis, it appears, about the year 1674, was the first to discover that the urine in diabetes possessed a sweet taste, and it was left till a century later (1778) for Cowley to isolate the saccharine principle. Sugar was subsequently detected in the blood. Towards the close of the eighteenth century, Rollo promulgated the notion that diabetes was due to an imperfect digestion—to a derangement, having its seat in the stomach, and resulting from a peculiar alteration in the gastric juice, which had acquired the pretended morbid property of changing into sugar the vegetable materials ingested. Later, however, Tiedemann and Gmelin showed that during the digestion of amylaceous matter,

sugar was naturally formed in the intestine; and, Magendie and others, subsequently proved that sugar likewise passed into the blood, whilst the digestion of amylaceous matter was going forward. It hence followed, that the formation and absorption of sugar in the digestive system from vegetable materials, could no longer be looked upon as the source of diabetes, because such processes were shown to take place as physiological occurrences. Bernard's investigations were at this stage commenced, and as his views have lately formed the accredited doctrine upon this subject, and were the means of eliciting my own researches, I shall have to refer to them in detail as I proceed.

Looking to the extensive distribution of sugar in the vegetable kingdom, and to its entering so largely into the constitution of the food of animals, there can be no doubt that it forms an exceedingly important material, both in regard to animal and vegetable life. Being soluble, and of a highly diffusible character, it readily passes, after ingestion into the animal system, by a physical process, from the stomach and intestine into the circulation. The action of the blood-vessels, in respect to absorption, is purely physical; whilst the special absorbent system of the intestine, as it commences in the villi, has a peculiar selective power, picking up certain materials (oleo-albuminous), and rejecting others, in a manner that cannot be explained by physics. Sugar, salines, and such like, pass, in conformity with the laws of endosmose and exosmose, into the blood of the portal system, and are carried to the liver before entering the general circulation. Now, from facts I shall have to bring forward, it appears that sugar is arrested in the liver, and converted by it into a material, which is found pretty largely in the healthy organ.

Besides sugar itself, there is another material, even more extensively existing in the vegetable kingdom, which,

although presenting far different properties to sugar, yet is transformed into it with the greatest rapidity when certain conditions are present. This substance—starch—is of no use, I apprehend, as such, either to the animal or the plant. It would seem to form a store of carbonaceous material, which may be kept for centuries without undergoing change; but which, when exposed to the conditions leading to its requirement, is with rapidity transformed into sugar—a material that can be made use of in the processes belonging to active life. A grain of wheat, excavated with the mummy belonging to ages past, retains its starch unchanged. Supply this grain of wheat with warmth, air, and moisture, and active life commences. The starch now immediately begins to be transformed; the condition that has created a demand for it, leads to its transformation into a body, which is afterwards susceptible of being appropriated to the wants of the growing organism.

In vegetable life, it is diastase—a principle developed during germination—which occasions the metamorphosis of starch into dextrine and sugar. It seems to me exceedingly doubtful, however, if starch pass on from dextrine into sugar during germination to the extent that is generally imagined. Certainly, the quantity of sugar contained in malt must not be taken as an index of the quantity of sugar actually existing in the germinating grain. I have allowed barley to grow in a temperate atmosphere, fully to the extent that is done by the maltster; and, on making a cold infusion of it, have obtained an astonishingly slight indication of the presence of sugar. On the infusion being warmed, however, or on the growing barley being exposed to a moderately elevated temperature—as is done in malting, on the kiln—a large production of sugar is observed to result. What I have said, refers only to barley that has germinated to the extent allowed in malting; for, it is true that, without

the application of artificial heat, when barley has grown much beyond this extent, a considerable quantity of sugar will be found to be contained in it.

In the animal system, there are secretions poured out by the digestive apparatus, which have the power of rapidly transforming starch into sugar. Human saliva converts starch into sugar with an astonishing rapidity, but it is curious, that the saliva of all other animals does not possess a similar property. It is a fact that I can attest from my own experience, that the secretions collected from the large salivary glands of the dog, either taken separately or mixed together, have no action whatever towards converting starch into sugar. It appears, however, that the fluid derived from the mouth possesses, to a slight extent, a transformative power. But, that the saliva is not the fluid specially intended for acting upon the amylaceous constituents of our food, is to be inferred from other considerations, besides the difference just alluded to, in the extent of power it enjoys, amongst different members of the animal kingdom. Starch is seldom ingested as food under such favorable circumstances for transformation, as it exists in our operations out of the body where a decoction is employed. The food is delayed but a comparatively short period of time in contact with the saliva in the mouth; and, supposing any action to take place in this cavity, it must be checked when the stomach is reached; for, it can be shown by experiment that our saliva exerts no transformative effect in the presence of an acid.

There is another secretion which is poured into the digestive tract further on, that seems to be specially intended for converting starch into sugar. The pancreatic juice at 90° to 100° rapidly occasions the conversion of starch into sugar; and, unlike what is the case with saliva, all the conditions in the interior of the system are favorable

for the exercise of its power. The food has been reduced to a semifluid state, so that it can become thoroughly incorporated with the secretions. The acid of the chyme has been more or less neutralized by the alkali of the pancreatic juice and bile. The temperature is elevated and equable; and, the passage along the intestine is slow, by which the starch is delayed in contact with its metamorphosing agent. Probably the secretion from Brunner's glands, and possibly that from the surface of the small intestine, assist the pancreatic juice, which, also has an office to perform in connection with the emulsification or preparation of fatty matters for absorption.

Such external sources of sugar, are all that were known previous to Bernard's time. Now, after an attentive examination of the circumstances connected with diabetes, Bernard was led to think that there might be something besides amylaceous matter—something unknown to chemists and physiologists, that might give rise to the production of sugar in the body. This, he particularly inferred from the fact, that the quantity of sugar escaping from a diabetic patient, under a mixed diet, was much greater than could be accounted for by the starch and sugar ingested; and that, although saccharine and amylaceous materials might be completely abstained from, still sugar did not cease to appear in the urine. Here was his motive for undertaking an investigation, out of which have sprung such unanticipated results.

In Bernard's first experiments, animals were kept upon a diet of a strongly saccharine character, with the view of ascertaining, how far the sugar could be followed in the circulatory system, after its absorption by the branches of the portal vein. His results led him to the conclusion that it might be detected as far as between the liver and the right side of the heart. To show that the sugar en-

countered at this point, was directly derived from the food, a counter-experiment—an experiment where starch and sugar were excluded from the diet—was made, with the expectation of obtaining a negative result. Instead of this, however, to Bernard's great astonishment, sugar was found as before; and thus was laid the foundation of the theory which gave to the liver a glyco-genic function. It was in the year 1848 that the discovery of this function was announced.

Looking at Bernard's celebrated experiment in the light that it has hitherto been regarded, nothing, certainly, would appear to be wanting to establish in the most conclusive manner the existence of a sugar-forming function in the liver. In the recently killed animal, that has been feeding for some time past on a purely animal diet, and therefore, that has been receiving no sugar into its system from an external source, the blood of the portal vein, or that on its way to the liver, is found devoid of sugar; whilst the blood of the hepatic veins, or that flowing from the liver, is highly charged with sugar. The liver itself, also, unlike any other organ of the body, gives signs of a strongly saccharine impregnation. Such evidence, really appears in unmistakable language to indicate that, for a destined purpose, sugar is formed by the liver, and carried away from it by the hepatic blood. And, from the extent to which the sugar is found, it might be reasonably inferred, that its production is connected with some important functional operation going on in the animal economy. Now, the evidence certainly establishes, beyond all question, that sugar can be produced in the animal system without being derived directly from the food; but the great point of interest in physiology, and likewise as regards the pathology of diabetes, is, whether this production is really *taking place during life*, in accordance with the inference that has been drawn from results,

which, be it observed, have been obtained after death. Does, in fact, the examination, as it has been hitherto conducted, afford us an indication of the *physiological state?* or, can the result of this examination be only taken for what it is really worth—namely, as the representation of a *post-mortem condition?* This is the point that I shall have specially to touch upon, after I have proceeded a little further, and brought the history of the subject up to the present period.

It was at first considered that the sugar found in the system of the animal feeder was formed from an albuminous material. Bernard's next step—and an important one it has proved—was to discover and isolate the substance, which is the source of animal glucose. In September, 1855, he communicated to the Academy of Sciences of Paris, his discovery, that the formation of sugar in the liver, might be shown to take place after death. A stream of water was passed through the vessels of a liver, removed from a recently killed animal, until the whole of the sugar impregnating its tissue was washed out, and it gave no reaction with the copper or fermentation tests. The liver was placed aside and examined again some time afterwards, when a strong indication of the presence of sugar was obtained. All consideration about the influence of any special vital process was here necessarily out of the question, and the production of sugar resolved itself, into the result of a simple chemical action.

The source of the sugar still remained to be disclosed; but it was evident, that it must consist of something existing in the liver, not so easily taken up by water as sugar itself, otherwise, it would have escaped from the organ with the latter. In 1857 the isolation of the sugar-forming material was announced, and as sugar-formation was supposed by Bernard, to constitute its physiological destination, it

was called by him, the Glyco-genic Matter of the liver. The glyco-genic theory was considered to remain unaltered by this discovery, except in so far, as it was rendered more complete, by the detection of a recognisable source for the animal glucose.

My own researches, of which I shall have presently to speak, have led me to look upon the term glyco-genic as objectionable, if applied to this newly discovered substance upon physiological grounds. It is true, that, after death, and under certain unnatural conditions, it is a sugar-forming substance; but, under natural circumstances, it does not seem intended for the production of sugar. Amyloid substance, is a name that has been given to it by some, on account of its alliance in chemical behaviour to starch. The term hepatine, was also, formerly, employed by myself to represent it, and was simply derived from its connection with the liver. In deference to a suggestion, however, on the recent publication of a communication in the 'Philosophical Transactions,' I avoided the multiplication of terms and used that of "amyloid substance." This, for the sake of consistency, I shall now continue to use, until our knowledge of the physiological relations of the body is more perfect, so that, it may be named upon scientific principles. Until then, it appears to me necessary only, that its provisional name should certainly be one, which is not exposed to the chance of leading to error, by implying a purpose, I have good reason to believe, it is not naturally specially intended for; such as would be the case with the word glyco-genic.

The situation of the amyloid substance in the liver, is in the hepatic cells—at least, such a conclusion is to be inferred, from what is learnt by a micro-chemical examination of the structure of the organ. As it is not present after death from disease, but is always present under healthy

circumstances, its production may be supposed to result, from the exercise of a specific functional activity of the liver.

In its chemical properties amyloid substance is allied to starch, but even more closely so to dextrine. When pure, it is a neutral, colourless, tasteless, and inodorous body, never presenting any other, than an amorphous granular appearance under the microscope. It is largely soluble, although not with rapidity, in water; and its aqueous solution presents an opaquely lactescent character. It is curious, that in the form of a highly concentrated solution it is transparent, the lactescence appearing only on dilution. It is insoluble in alcohol and glacial acetic acid, by which agents, it may be thrown down from its aqueous solution. It is unaffected at a boiling heat by caustic potash. It is devoid of nitrogen, and is composed of C_{12} H_{12} O_{12}. With iodine, its behaviour is like dextrine, producing a deep wine-red coloration. Its most important property, is its susceptibility of transformation into sugar. It does not itself react with the cupro-potassic or fermentation tests; but, after being boiled for a short time with a mineral acid; or, after contact with many animal products, as saliva, pancreatic juice, blood, liver-tissue, &c., at a moderately elevated temperature, the characteristic reactions of glucose are discoverable.

For studying the relations of the amyloid substance, it may easily be procured by the following process. A piece of liver, from a recently killed healthy animal, is to be plunged for a few minutes into boiling water, for the purpose of suddenly checking any further loss of material from transformation into sugar. The hardened liver is then pounded in a mortar, mixed with a fairish quantity of water and boiled, so as to make a decoction. This decoction, after being strained or filtered, is to be poured into

five or six times its bulk of alcohol. The precipitate consists of the amyloid substance. It has now, only to be collected on a filter, and washed with alcohol and dried.

The amyloid substance, obtained as above, is free from sugar, and contaminated only to a slight extent with other impurities. I have found it may be obtained on a large scale and much more economically, although not quite in such a state of purity, by passing a stream of water through the vessels of a recently removed liver to wash away the sugar that has been formed in it, then, making a decoction of it, and, after straining, evaporating the decoction gradually down and collecting the scum which forms layer after layer on the surface. This scum consists of amyloid substance, and is tolerably pure, when it has been drained as much as possible of the fluid in contact with it. As thus prepared, it is of a soft and pulpy or glutinous consistence, of a sticky nature, and of a somewhat pale-yellowish colour. Spread out and allowed to dry spontaneously, it forms a semitransparent, hard, brittle, and gum-like or resinous-looking body.

To render amyloid substance chemically pure, it should be dissolved and boiled in a solution of potash. It is, then, reprecipitated with alcohol and washed, by which it is freed from all adhering foreign organic matter. But, it is now found to cling to the alkali used, with such tenacity, that this cannot all be removed by washing. It is, therefore, necessary to dissolve again in water and neutralize with acetic acid. The acetate formed, and the excess of acetic acid used, are easily separated by a further employment of alcohol in a similar manner as before.

In making the quantitative determinations of amyloid substance in the liver, that presently follow, I availed myself of its properties, of resisting the action of a boiling solution of potash, and of being precipitated by spirit. A piece of

liver (about 200 grains) was weighed and pounded in a mortar with about two thirds of its weight of solid caustic potash. A little water being added the whole was carefully collected and transferred to a small porcelain capsule and boiled for several minutes, until all was completely dissolved. The liquid was then poured into about six times its volume of spirit, by which the amyloid substance was thrown down as a white flocculent precipitate. The precipitate after being well washed with spirit was dried and weighed.

From a most extensive employment of this process, I have found it easy of application and susceptible of a considerable degree of minuteness as to accuracy. The precipitate does not consist quite wholly of amyloid substance, but the amount of other matter present, when the analysis is properly conducted, is so small that it may be fearlessly left out of consideration, as far as our physiological researches are concerned.

I regard it as a most interesting and significant feature in connection with the history of the amyloid substance, that it can be shown to be directly produced in the liver as a result of the ingestion of sugar and its ally starch. Whilst prosecuting my researches upon the relations of sugar in the animal system, I found, that the size of the liver was to a most striking extent influenced by the nature of the diet; and, that the alteration thus induced was chiefly, if not entirely, due to the amount of amyloid substance that was present. I was first conducted to the discovery of these facts, when, in my early experiments, I was seeking to determine if the quantity of sugar found in the liver and blood of the dog after death was altered from its ordinary amount by the previous administration of a strictly vegetable diet. The process I then adopted for examining the liver was to remove and weigh it, and then to pass a stream

of water through its vessels, until its tissue was completely deprived of sugar. After a vegetable diet, I noticed, first, that the liver was of enormous size, in comparison with what I had been accustomed to meet with under an animal diet; and secondly, that there was a remarkable quantity of a material present, which much interfered with my analysis, and which I subsequently found to be amyloid substance.

From the series of observations to be given, it would appear, that the liver of the dog may, in a short space of time, be doubled in weight, by a vegetable diet, and nearly so, by an animal diet with a considerable admixture of sugar. As a strictly animal feeder, the liver of the dog, from an average of eleven examples, nearly equalled half an ounce to every pound the animal weighed. Under a strictly vegetable diet, the average proportion given by five examples, exceeded an ounce to a pound; whilst, upon an animal diet with an admixture of sugar, the figures resulting from four observations indicated, within a fraction, the proportion of an ounce to the pound. The estimation of the amount of amyloid substance present, that was made in most of the examples, shows, in how striking a degree, the extent of production of this material is influenced by the nature of the food.

In all the observations that follow, the life of the animal was suddenly destroyed, its body then opened, and the liver removed. The liver thus circumstanced, immediately drained itself, by the contraction of its vessels, of the principal portion of its blood; whereby was avoided, those differences as regards degree of congestion, which are observable in the human subject, where the examination is not made until some hours after death.

The observations are given without a single exception, just as they presented themselves under their separate

heads; and further, the animals were taken, as they happened to be brought to me, without any selection, except such as was needed for the vegetable and saccharine diets, many dogs refusing to partake of such kind of food. It will be understood, that they were all, as far as could be judged, in a perfect state of health at the time of being killed. The weights are of the avoirdupois scale. The dogs were weighed just before death, and the weight of the liver is without the contents of the gall-bladder. In most of the instances, life was destroyed, a few hours after the administration of food.

The Liver of the Dog under a Diet of Animal Food.

The allowance of food for these dogs, with the exceptions to be mentioned, was one of the ordinary penny bundles of tripe *per diem.* In examples Nos. 7 and 8 an extra allowance was purposely given, to ascertain if any material variation in the result would be occasioned. No. 7 consumed two bundles of tripe daily for four days prior to its death. No. 8 was allowed its full tether, and the quantity of tripe it devoured was truly enormous; three bundles one day, four another, five the third, and two and a half the day it was killed. As will be observed, the figures yielded, do not dislose, any significant deviation from the average results.

	Weight of dog. lbs.	oz.	Absolute weight of liver. oz.	Relative weight of liver to animal.
No. 1	15	8	$7\frac{3}{4}$	1 to 32
No. 2	12	0	$7\frac{1}{4}$	1 to $26\frac{1}{2}$
No. 3	11	$14\frac{1}{2}$	$6\frac{1}{8}$	1 to 31
No. 4	15	10	$7\frac{3}{4}$	1 to 32
No. 5	11	0	$6\frac{1}{8}$	1 to 29
No. 6	11	$15\frac{1}{2}$	$6\frac{1}{2}$	1 to $29\frac{1}{2}$
No. 7	15	$5\frac{1}{2}$	$8\frac{3}{4}$	1 to 28
No. 8	24	$4\frac{1}{2}$	$11\frac{3}{4}$	1 to 33
No. 9	14	$9\frac{1}{2}$	$7\frac{1}{8}$	1 to 32
No. 10	17	0	$8\frac{7}{8}$	1 to $30\frac{1}{2}$
No. 11	9	8	$7\frac{1}{4}$	1 to 21
Total	158	$11\frac{1}{2}$	$85\frac{1}{4}$	1 to 30

The average of these eleven examples of dogs, restricted for some days prior to death to an animal diet, thus gives to the liver a weight equal to the $\frac{1}{30}$th part of that of the animal. Stating it in other words, there is very nearly half an ounce of liver for every pound the animal weighs.

On casting the eye through the list, it will be seen, that in neither instance, is there any very striking departure from the average, except perhaps in No. 11, which consisted of a three-parts grown pup. But even here, where the highest proportion I obtained was yielded, the relative weight, as will be presently apparent, falls very far short of what was found, after a vegetable diet, and also, after an animal diet with the addition of sugar.

Looking to the amount of amyloid substance present in these livers, my analyses have yielded the following results: —In seven out of the eleven examples, examinations were made under precisely similar circumstances. The liver was removed and weighed immediately after death, and a piece

at once taken for analysis, so as to avoid, more loss than was possible, from *post-mortem* transformation into sugar.

Amount of Amyloid Substance in the Liver, yielded by the foregoing examples upon a Diet of Animal Food.

	per cent.
Example No. 3	8·29
Example No. 4	5·24
Example No. 7	5·61
Example No. 8	8·45
Example No. 9	4·88
Example No. 10	10·95
Example No. 11	6·94

Average amount of amyloid substance yielded by the above seven analyses, 7·19 per cent.

The excluded examples are thus accounted for. In example No. 1 no analysis was made. In No. 2 the liver was left in the animal for 2½ hours before it was examined. It then yielded 3·37 per cent. of amyloid substance. In No. 5 the liver was left in the animal for ten minutes and then removed, but not examined for the space of two hours. It then yielded 3·51 per cent. of amyloid substance. In No. 6 an error occurred in conducting the analysis, which rendered it useless.

The Liver of the Dog under a Diet of Vegetable Food.

To ascertain the effect of a vegetable diet on the liver, I submitted five dogs to a course of food for several days previous to death, consisting of barley-meal and potatoes, or, where this was refused, to bread and potatoes. The following are the results that were obtained :—

	Weight of dog. lbs.	oz.	Absolute weight of liver. oz.	Relative weight of liver to animal.
No. 12	17	8	$19\frac{1}{4}$	1 to $14\frac{1}{2}$
No. 13	11	8	$12\frac{1}{2}$	1 to $14\frac{1}{2}$
No. 14	15	8	$11\frac{3}{4}$	1 to 21
No. 15	18	10	28	1 to $10\frac{1}{2}$
No. 16	17	5	$12\frac{1}{4}$	1 to $22\frac{1}{2}$
Total	80	7	$83\frac{3}{4}$	1 to 15

Taking the average of these five instances as a sample of the effect of a vegetable diet on the liver of the dog, it appears that the organ rather more than equals in ounces the number of pounds the animal weighs. It will be remembered that, after an animal diet, the average given was rather under the half ounce to the pound. The facts as to the amount of amyloid substance stand thus:—

No analyses were made of the livers of the dogs Nos. 12 and 13, but the quantity of amyloid substance was unusually large. It was whilst examining these livers for the determination of sugar that I was first led to notice the effect of a vegetable diet that I am now describing.

Amount of Amyloid Substance in the Liver, yielded by the foregoing examples upon a Diet of Vegetable Food.

	per cent.
Example No. 14 . . .	9·87 *
Example No. 15 . . .	25·30
Example No. 16 . . .	16·50

Average amount of amyloid substance yielded by the above three analyses 17·23 per cent.

* This liver was not examined till an hour and a half after death.

The Liver of the Dog under a Diet of Animal Food, with an Admixture of Sugar.

The effect of giving an admixture of sugar with animal food is similar to that produced by a vegetable diet. The sugar employed in my experiments was the brown or moist sugar that is used for domestic purposes. Various devices had to be resorted to, to get the animal to take it. The plan I found to succeed the best was to introduce it into short lengths of the intestinal tube forming part of the bundle of tripe. I will give the leading particulars belonging to each of the four dogs that formed the subjects of experiment in this way.

No. 17. A nearly full-grown mongrel dog, kept for eight days on a diet consisting of sugar and a bundle of tripe *per diem*. At first, one-third of a pound of sugar was administered daily, but after three or four days, the animal showed a disinclination for food, vomited, and had bilious diarrhœa. The quantity of sugar was reduced to a quarter of a pound daily. The dog now devoured voraciously all that was given to it. The urine collected from the bladder after death gave a strong reaction of sugar (grape-sugar).

No. 18. A youngish dog, fed for nine days on a bundle of tripe and a quarter of a pound of sugar daily. It consumed its food well at first, but during the last few days a great amount of coaxing was required to get it to take its full allowance. There was scarcely any urine to be procured from the bladder after death; what there was, gave no saccharine reaction.

No. 19. A middle-aged dog, kept for eight days on a bundle of tripe and a quarter of a pound of sugar daily. The urine at death gave a slight but decided reaction of sugar.

No. 20. A dog not quite full grown. Fed for five days on the same diet as the preceding dog. The urine collected after death gave a strong reaction of sugar.

The following are the results of the examination made in these cases :—

	Weight of dog. lbs.	oz.	Absolute weight of liver. oz.	Relative weight of liver to animal.
Example No. 17 . .	10	3	12	1 to 13½
Example No. 18 . .	11	14	12¾	1 to 14½
Example No. 19 . .	17	11	10¾	1 to 26
Example No. 20 . .	12	0	13½	1 to 14
Total	51	12	49	1 to 16½

The average yielded by these four dogs, thus gives a relative weight of liver, bearing a close approximation to that obtained after a diet of vegetable food; in the one case being as 1 to 16½, in the other as 1 to 15. The amount of amyloid substance in each example was large, as is shown by the following results of analysis :—

Amount of Amyloid Substance in the Liver yielded by the foregoing examples upon a Diet of Animal Food and Sugar.

	per cent.
Example No. 17 . . .	12·80
Example No. 18 . . .	17·55
Example No. 19 . . .	12·33
Example No. 20 . . .	15·37

Average amount of amyloid substance yielded by the above four analyses 14·5 per cent.

Besides these above-mentioned effects upon the liver which resulted from the admixture of sugar with animal food, the organ likewise presented an alteration in

its physical appearance. Under a purely animal diet, it was comparatively firm and fleshy, requiring considerable force to break it down between the fingers. Under the animal diet with sugar, it became exceedingly soft, and was readily crushed by a very slight pressure. It looked swollen and flabby and was pale in colour. The bile I noticed, too, was of a much paler yellow than after a strictly animal diet.

There was another fact, that, in each case, I was struck with, on making the examination after the diet of sugar and animal food. Peyer's patches and the solitary glands of the intestine, especially those of the cæcum, presented a prominence and an appearance of vascularity that conspicuously excited my attention.

In three out of the four examples, as mentioned in the particulars, the allowance of sugar that was given, communicated to the urine a saccharine quality. It is worthy of note, that, although cane-sugar was ingested, it was grape-sugar that was detected in the urine.

The conclusion to be drawn from the observations I have made upon the rabbit in reference to the state of its liver is, that the better the condition of the animal, the larger is the quantity of amyloid substance that the organ contains. The condition in which most of the London rabbits are sold is not one from which, there is reason to conclude, a fair estimate can be made of the relative size of the liver and proportion of amyloid substance belonging to the perfectly healthy and vigorous state; for, according to my experience, it is quite the exception on opening the abdomen, not to meet with a liver, more or less pervaded with entozoa. Subjoined, however, are the results of four examinations, the rabbits having been kept upon their usual food, and having presented about an ordinary appearance as regards health and condition :—

	Weight of rabbit. lbs.	oz.	Absolute weight of liver. oz.	Relative weight of liver to animal.	Amount of amyloid substance. per cent.
No. 1	5	3	$3\frac{3}{8}$	1 to 24	7·5
No. 2	3	15	$2\frac{5}{8}$	1 to 24	6·69
No. 3	7	8	$3\frac{3}{8}$	1 to 33	3·16
No. 4	5	11	$3\frac{1}{4}$	1 to 28	12·59

When looking at the relative weight of the liver in the rabbit, it must not be left out of consideration, how large a quantity of perfectly extraneous matter is estimated with the weight of the animal. The intestinal canal is exceedingly long, and the contents, especially of the stomach and the very large cæcum, exceedingly bulky. The stomach and intestine, in fact, with their contents, in two instances that I specially examined, were found to equal the 1-5th and the 1-$5\frac{1}{2}$th part of the weight of the body. In the dog, the proportion is very much less. In one dog that had been kept on a vegetable diet, the alimentary canal and its contents formed about the 1-8th part of the weight of the animal; whilst, in another that had been well fed on an animal diet, the proportion was only the 1-10th.

I have conducted experiments on rabbits, in which, instead of allowing them their ordinary food, I have kept them for a few days on a mixture of starch and sugar only. Other rabbits were kept fasting for the sake of having a comparison. In these experiments, the effect produced on the liver by the administration of the starch and sugar is strikingly corroborative of the results obtained upon the dog, under the influence of different diets.

In one experiment, a couple of full-grown rabbits were taken as nearly as possible resembling each other. One was kept fasting, whilst the other was fed daily for three days, through a tube passed down the œsophagus into the stomach, with one ounce of starch and three-quarters of an ounce of

grape-sugar made into a semifluid mass with water. On the fourth day both animals were killed.

	Weight of animal. lbs. oz.	Weight of liver. oz.	Amount of amyloid substance. per cent.
Rabbit fasting	3 1	$1\frac{2}{5}$	1·3
Rabbit on starch and grape-sugar	3 4	$2\frac{4}{5}$	15·4

Another experiment was made on two half-grown rabbits, likewise as nearly as possible resembling each other. One was made to fast, whilst the other was fed on an ounce of starch and the same quantity of cane-sugar daily for three days. On the fourth day the examination was made.

	Weight of animal. lbs. oz.	Weight of liver. oz.	Amount of amyloid substance. per cent.
Rabbit fasting	1 14	1	1·4
Rabbit on starch and cane-sugar	1 $14\frac{3}{4}$	$2\frac{3}{8}$	16·9

I have a record of a third experiment, but in this, the amount of amyloid substance was all that was determined. The rabbit was allowed to take its ordinary food, and in addition, three-quarters of an ounce of loaf-sugar and half an ounce of starch were administered daily for three days. The animal was killed on the fourth day. The liver was not analysed until the day after death, but it then yielded 22·7 per cent. of amyloid substance.

After the administration of sugar, as in the above experiments, the liver is altered in its physical appearance, like what has already been observed in the case of the dog. It becomes of a very pale colour, and so soft that it is readily broken down by slight pressure between the fingers. I have seen it, indeed, so softened as to be almost

pulpy, scarcely holding together when taken up by a pair of forceps.

All these results, both upon the rabbit and the dog, are so striking and so corroborative of each other, that it seems to me, nothing further can be required, to prove that the sugars and starch are made use of by the liver for the production of amyloid substance. The marked variation observed in the size and constitution of the liver under different diets, is certainly a point of much physiological interest, and possibly may ultimately be found to present an important bearing in connection with medical practice.

The fact that the amyloid substance should be formed in the liver from sugar does not of course, taken alone, afford an actual proof of anything against the glyco-genic theory; but, upon *à priori* grounds, that the amyloid substance should be intended to come back again into sugar, certainly seems, to say the least, improbable. Such a process as the conversion of sugar into amyloid substance, and amyloid substance back again into sugar, I think will be admitted, certainly does not accord with what might be expected, from the notions we have a right to entertain of the manner in which the operations of Nature are conducted. But there is another, and a more significant aspect under which this question presents itself, and this I shall now proceed to examine.

Although the glyco-genic theory of Bernard has been so universally admitted, still it has had its adversaries; but nothing, that I am aware of, has been brought forward resting upon grounds at all approaching in character those I have been gradually led from experimental research to advance.

M. Schmidt, in 1849, without being aware of Bernard's discoveries, compared the sugar found in the blood to urea, and thought it took origin in all parts of the circulatory system, without any special organ to form it.

M. Figuier has contended, that there is no sugar in animals unless introduced from without through a saccharine or amylaceous alimentation. In the case of the carnivora, he says, the flesh coming from the vegetable feeder contains sugar. Again, he considers that sugar, instead of being formed by the liver, is merely stored up by this organ, being carried to it in the portal blood. The reason he gives for no sugar being detected in the portal blood is, that some unknown matter exists there, which masks its presence. Such statements as these were soon conclusively shown by Bernard to be contrary to the results of direct observation; and, the attack of M. Figuier, by the unstability of its foundation, ended, in stregthening rather than otherwise the position of the glyco-genic theory.

More recently, M. Sanson has followed in much the same direction as that taken by M. Figuier. He considers there is only one source for the sugar met with in the animal economy, and that this one source is exterior to the body; that the glycogène, or sugar-forming substance of Bernard, is nothing but dextrine, which, he says, does not belong specially to the liver, but is found in the blood and the various textures of the body; that the liver is not an organ for producing the glycogène, but simply for abstracting it from the blood, and transforming it into sugar with greater activity than is elsewhere effected. Bernard's experiments, he considers, prove only that sugar is absent in the blood going to the liver, and present in that coming from it; and not, that the change of glycogène into sugar is the result of a process of secretion. He looks upon it as a simple chemical change, because it also takes place after death.

These views of M. Sanson have not been sustained by other observers. In the experiments bearing on his statements, that I myself have conducted upon carnivorous animals; although, I have found that a small quantity of

a substance, which, like the amyloid substance, is susceptible of transformation into sugar under the influence of saliva, and also of being coloured red by iodine, can be extracted from lung and muscular tissue; yet I have failed in obtaining it from either the blood, spleen or kidney. I cannot agree, therefore, to this substance having the extensive distribution through the system, represented by M. Sanson; and, certainly after a large experience upon the examination of blood under the most varied circumstances, I have never observed it become more saccharine than at first, on being allowed to repose after removal.

In the position of antagonism to the glyco-genic theory, that my researches have compelled me now to occupy, I make no attack upon the accuracy of Bernard's experimental results. I fully admit that, confining our attention to the investigations on the subject, as they have been hitherto conducted, the evidence obtained, certainly appears satisfactorily enough to establish the existence of a glyco-genic function in connection with the liver. For example, an animal is kept for some time on food devoid of sugar or sugar-forming principles. An examination after death shows that a considerable quantity of sugar is to be found in the blood escaping from the liver through the hepatic veins, whilst none, or, *speaking more precisely*, next to none, is to be detected in the blood on its passage to the organ through the portal vein. Further, the liver is the only organ of the body in which sugar can be discovered, and here, it is to be recognised to a decidedly notable extent. Further even yet, the sugar-forming substance is to be readily isolated from the liver, and the process of sugar-formation to be satisfactorily accounted for.

What, then, seemingly, can be wanted more convincingly to establish the function of glyco-genesis? and what can possibly be said against a theory founded on facts so strik-

ing and apparently so conclusive? I unhesitatingly admit, that my experience stands in confirmation of the facts as they have been described by Bernard. I do not, as I have before said, for one moment, impeach the accuracy of the results that have been mentioned. The question that I have, step by step, been led by experimental evidence to raise, is, whether these results have received their just interpretation? —whether the premises upon which Bernard's conclusions have been formed are not fallacious in the light under which they have hitherto been taken? To proceed more openly to the point, the position of the matter is this: experiments have been performed, and results obtained, which are of so striking and decided a character that no one can entertain a doubt of their validity. But, these results have been derived from *post-mortem* examinations, and taken to represent the *ante-mortem* state. Now it happens, as can be shown by a different mode of experimenting to what has previously been adopted, that the two do not correspond; and here is my ground of opposition to the glyco-genic theory. A physiological occurrence has been inferred from a condition observed after death, and the inference proves to be fallacious.

The two main points for our consideration are: as to the escape of sugar from the liver by the blood, and as to the natural state of the liver itself during life.

From the old method of procedure in experimenting, it was accepted as established, that the blood belonging to that part of the circulation intervening between the liver and lungs, was highly charged with sugar as a physiological condition; whilst that belonging to the arterial system had become almost completely deprived of all saccharine principle. Now, the results for arriving at these conclusions were obtained by collecting a specimen of blood from an artery of a living animal; and then, after the sudden destruction of life, procuring another specimen from either

cavity of the right heart, or from the inferior cava, and submitting the two to an analytical examination. But, unless certain precautions are observed, the necessity for which was not formerly known, such a method of experimenting must inevitably lead to a fallacious physiological induction. The knowledge required is of the state belonging to life, and this we fail to obtain unless we operate on blood, cardiac as well as arterial, that has been collected before, or at the very instant of death.

It was, whilst conducting some experiments with the view of ascertaining the nature of the change the sugar underwent in its supposed destruction on its passage through the lungs, that I was first led to call in question the substantiality of the glyco-genic theory. I was endeavouring to determine if the supposed destruction would occur, on injecting recently abstracted right-ventricular blood through the capillaries of the artificially inflated dead lung. During February, 1854, my inquiry induced me to withdraw blood by a catheter from the right ventricle of the living animal, to perform, so to speak, catherism of the right heart—an operation that may easily be accomplished without creating any injury or disturbance, by passing a properly curved instrument down through the right jugular vein and superior cava into the heart. I was astonished to find that the blood thus removed presented in each case a strikingly different behaviour to what had hitherto been considered as naturally belonging to it, for it was scarcely at all impregnated with saccharine matter. This observation did not at first receive from me the importance that was due to it; but in June of the same year, according to my note book, I returned to the subject, and in an experiment, again observed, that a specimen of blood removed from the right ventricle during life contained scarcely a trace of sugar; whilst, after the subsequent destruction of life, the blood which flowed from an incision

into the right heart gave an indication of a copious saccharine impregnation.

Notwithstanding that such results appear now so striking, yet I did not, even after the last observation, give to them their proper significance. My mind was so strongly impressed with the notion that the glyco-genic doctrine was indisputably established—so strongly impressed with the prevailing conviction that sugar was extensively formed by the liver during life and poured into the circulation through the hepatic veins, that I was for some time disposed to think there must have been some fallacy in my experiment. I felt inclined, indeed, to think that the catheter had not been fairly introduced into the heart, or had come in contact with the current of blood descending through the superior cava, rather than discredit our previous views, and no longer regard the strongly saccharine state of the blood met with so soon after death, as a representation of its natural or *ante-mortem* condition.

Reflection, however, began to excite mistrust, and the necessity of deciding the cause of my results then presented itself strongly before me. Numerous experiments were now performed with the greatest care, from which I thoroughly satisfied myself there was no source of fallacy, where fallacy I had thought might exist; and from which, also, I learnt in the most unequivocal language that, what had lately been regarded as the natural state of the blood of the right side of the heart, was not in reality such. Blood collected from the heart as was formerly done, does not behave like the blood removed during life, and, therefore, an inference that has been drawn of the condition of the one, from an examination of the other must be discarded as erroneous.

I feel that I need not speak upon this subject with the slightest degree of hesitation. From the large number of experiments that I have made, I think I may assert without

fear of subsequent contradiction that, contrary to what has been recently believed, sugar is only found to the extent of the merest trace—to no greater an extent than it is met with in the arterial system—in the blood of the right side of the heart under *a natural or ordinary condition during life.* The reaction with the copper solution, which, it must be remembered, is an exceedingly sensitive test, is sometimes so slight as to be liable to be overlooked altogether, unless special attention be given to the examination, as by placing the test-tube aside for some time, to allow any minute floating particles of sub-oxide to subside.

The following quantitative determinations conducted upon blood derived from the dog show that, although there may exist but a trace of sugar in the blood during life, yet, that sugar may be found to a notable extent in the blood derived from an ordinarily conducted examination of the same animal after death. We have here a proof of the difference I have insisted upon, between the blood belonging naturally to life, and that removed ordinarily after death—a difference, as far as I am aware, that has never been alluded to by others. Looking to the column showing the results of analysis of the blood removed after death, it will be observed that in the five instances, the proportion of sugar varied from half a grain to nearly one grain per cent.; there having been only a trace of sugar in the arterial and right-ventricular blood collected during life. Four out of the five livers belonging to the animals were submitted to analysis, and, as shown, were richly charged with sugar:—

Blood from the carotid artery and likewise from the right ventricle during life.	Blood (defibrinated) from the right side of the heart after death.	Liver a short time after death.
	per cent.	per cent.
No. 1. Trace of sugar.	$\frac{7}{10}$ths gr. of sugar.	Not analysed.
No. 2. Trace of sugar.	$\frac{65}{100}$ths gr. of sugar.	4·10 grs. of sugar.
No. 3. Trace of sugar.	$\frac{5}{10}$ths gr. of sugar.	3·39 grs. of sugar.
No. 4. Trace of sugar.	$\frac{94}{100}$ths gr. of sugar.	2·45 grs. of sugar.
No. 5. Trace of sugar.	$\frac{7}{10}$ths gr. of sugar.	2·44 grs. of sugar.

At the time the above analyses were made, I looked upon the amount of sugar in the right-ventricular blood removed during life, as too small for exact determination. I have since, however, found that although minute, yet, the quantity is with care susceptible of being ascertained. Defibrinated blood is poured into spirit, and the precipitate thoroughly washed with this agent. The filtered liquid is evaporated to a small bulk, and the sugar then estimated with the copper solution. In three instances, I found the quantity of sugar indicated, to be $\frac{47}{1000}$ths, $\frac{58}{1000}$ths, $\frac{73}{1000}$ths of a grain per cent. These results may be taken as affording an average representation of the per-centage of sugar normally existing in the right-ventricular blood of the living dog; for the specimens behaved, as regards degree of reaction, on being tested in the usual way with the copper solution, in a similar manner to those belonging to the other examinations I have made. In the first two instances, the blood was procured within a few hours after the animals had received food; whilst in the third, twenty-four hours had elapsed since food had been ingested.

To obtain a correct representation of the amount of sugar naturally belonging to the blood during life, it is necessary that the animal should remain in as perfect a state of tranquillity as possible during the performance of the operation of removal. Very slight causes are sufficient to determine an increase of sugar in the blood of all parts of the system. It is astonishing how immediate is the effect of a disturbance, produced by muscular efforts of resistance, or by embarrassment of the breathing. According to the state of the animal during the removal of the blood, I can predicate with confidence the kind of behaviour as far as regards sugar that the blood will present. Should there be any extensive amount of muscular disturbance, or embarrassment of the breathing, a considerable indication of the presence of sugar is sure to

be met with. Indeed, by simply interfering with the proper performance of the breathing, I have determined such a presence of sugar in the circulation as to have rapidly occasioned a strongly saccharine state of the urine. The supply of air was reduced short of producing asphyxia, and the urine in an hour's time became highly charged with sugar.

I believe that the unnatural flow of sugar into the circulation, which takes place under the circumstances above mentioned, can be satisfactorily accounted for. By mechanical compression of the liver, from violent contraction of the muscles of the abdominal parietes; or, from congestion of the circulation, through an impediment to the freedom of the breathing, it is not unreasonable to assume that conditions are established which will unnaturally tend to promote the escape of amyloid substance from the liver-cells, and its admixture with the blood. With such an occurrence, a production of sugar is the immediate result; but these are points that I shall have specially to advert to in a subsequent part of this work.

The circumstances I have alluded to afford an explanation, why blood removed from the carotid artery immediately after exposing the vessel, is ordinarily found in a marked degree more saccharine, than when collected ten minutes or so after the operation of exposing the artery has been effected. From the contiguity of the carotid artery to the pneumogastric nerve, picking up the vessel is almost universally attended with considerable resistance, and disturbance of the breathing. But, when the artery has been once fully separated from its adjacent structures, and a ligature placed around it, it can be easily drawn out and blood collected from it without causing any fresh resistance or disturbance.

I have, upon a few occasions, found blood removed from the carotid artery, giving a shade stronger reaction than the blood previously withdrawn from the right side of the heart.

I consider this to have been due to the different effects of the respective operations required. Exposing the jugular and passing a catheter into the heart, is not so likely to occasion resistance and disturbance as exposing the carotid.

The administration of chloroform must be always avoided when a specimen of blood is required in a natural state as regards sugar. Chloroform not only reacts of itself with the copper solution, but through its influence on the system determines, as I shall show further on, an unnatural flow of sugar into the blood.

The only point I know of in which Bernard's *results* and my own stand in opposition to each other is, as to the state of the blood collected from the right ventricle during life. In his 'Leçons de Physiologie Expérimentale,' vol. i, p. 121, he records an experiment made before his class, in which the blood removed from the right side of the living heart gave a neat reaction with the copper solution, whilst the blood of the carotid artery and jugular vein of the same animal gave no reaction at all. I do not doubt that we have here an accurate description of what was noticed, but it is directly at variance with the result I am confident in obtaining myself, and which I am frequently showing to others. If an extensive experience upon this particular point, may be advanced as affording reliable information, this experiment of Bernard must not be taken as a representation of the natural condition. A fallacy may have arisen from the animal not having been in a tranquil state, and nothing was formerly known, about the differences I have pointed out, that are observable in the blood under different conditions. Certain it is, according to my own experiments *on the living animal*, that sugar does not exist to a larger extent in the blood of the right side of the heart, than to that, which has hitherto been recognised as belonging to the blood

of the arterial and the general venous systems; and that, should sugar appear to any amount in the right-ventricular blood, it will also be found to a corresponding extent in that of the carotid artery and jugular vein.

With the knowledge we now possess, it is easy to operate in such a manner, as to enable us to alight upon the blood after death, presenting the condition that is natural to life. When an animal is killed, and, even only a short time allowed to elapse, before the chest is opened to collect the blood from the right heart, it is found strongly saccharine. But, if life be instantaneously destroyed, and the chest as rapidly as possible opened, and the heart quickly ligatured at its base and excised, then, the contents of its right cavities, will be found as free from sugar as if catheterism had been performed under normal circumstances during life. Here, the steps of the operation are so rapid, that time is not given for the *post-mortem* effects on the liver and the circulating fluid to be manifested.

I think, perhaps, that this is the most preferable mode of proceeding, to obtain a representation of the blood in its natural state. When the experiment is expeditiously and properly performed, the result can scarcely be liable to be exposed to the influence of any accidental disturbing circumstances. It is for this reason, that I can reckon with more certainty, on finding the blood of the right side of the heart in a perfectly normal state, after the sudden destruction of life and instant excision of the heart, than on removal through the catheter during life. The object of the *expeditious* excision of the heart will fully appear, when it is taken into consideration, as I shall presently mention, that sugar is formed in the liver with a surprising rapidity after death, and that the continuance of the circulation which occurs for a short time after the animal has been killed, quickly distributes this sugar through the system, carrying

it first to the right heart, and then through the lungs into the arteries.

One of the principal arguments in support of the glycogenic theory has been that, under a diet of animal food, the blood going to the liver is devoid of sugar, whilst that flowing from it is highly charged with sugar. I have spoken of the blood escaping from the liver, and have urged that the saccharine condition of it which is found after death is not to be taken as a representation of its natural or physiological state. As to the blood on the other side of the liver, I have recently given it a careful comparative examination. In three consecutive experiments, in which I scrupulously observed every precaution to obtain the specimens precisely in the condition that is natural to life, I could not perceive the slightest discoverable difference of behaviour between the blood of the portal vein and that of the right side of the heart. The portal blood gave the same trace of indication with the copper solution as I obtained with the cardiac contents. It was impossible, in fact, with the test re-agent to recognise any perceptible distinction between the two.

An explanation may naturally be looked for, to account for what has just been mentioned, being reconcilable with the current statement of physiologists, that under an animal diet the portal blood is to be found free from sugar. The fact is, that blood is examined (certainly by myself) much more closely now, than when such a wide difference was considered to exist in it, as it belonged to different parts of the circulation. A trace of reaction is now looked after and taken note of, that would have formerly been allowed to pass over unobserved. For instance, on well boiling a specimen derived from portal blood with the blue liquid, there is no decided reduction at first to be perceived. The test appears to have given no reaction, and such would have been formerly

described as the result. Had the test-tube been placed aside, however, and examined again in the course of a short time, just a traceable amount of red oxide would have been found, according to my recent experience, to have subsided.

As far, then, as we learn from what has preceded, there is not, as a natural process of life, that flow of sugar into the circulation from the liver, for the purpose of destruction in the lungs, which the former mode of experimenting led physiologists to believe. After death, and under certain unnatural states during life, it is true, there is a large escape of sugar from the liver; but, as a normal condition, there is only a trace of sugar in the blood between the liver and lungs, and this trace is also met with, on the other side of the lungs, in the blood returning from the system at large, and even in the blood on its passage to the liver. The blood, therefore—namely, that returning from the liver—which was formerly looked upon as affording evidence of the exercise of a glyco-genic function in the animal system, has nothing special belonging to it. The same character is met with, as far as I can discover, to precisely the same extent, in the blood of every part of the system.

Quitting now the blood, I shall next direct attention to the organ itself, which has for some years past been enjoying its reputed glyco-genic function.

Although I had discovered the facts I have pointed out respecting the blood, yet I still continued to look upon the liver as being strongly impregnated with sugar during life. Analysis of the organ in an ordinary manner after death, displayed an abundance of the saccharine principle, and I had nothing yet before me to lead me to suppose that this was not the true representation of the living state. At a loss, however, to account for the blood escaping from the liver, not being charged with sugar during life, as it is after

death, I began to seek for a cause, and thus arose the investigations to which I am now about to refer.

Observation up to this period tending to show that sugar was naturally contained in the hepatic tissue, the first idea that suggested itself, was, to look to those conditions of the circulatory system, which might favour or prevent the escape of the saccharine principle from the organ. I performed experiments upon the injection of defibrinated blood through the liver, at different degrees of pressure, immediately after death. But, failing to elicit anything satisfactory, it next occurred to me as possible that the liver, like the blood issuing from it, might be free or almost completely free from sugar during life. I did not at first look upon such a supposition as bearing any degree of probability; indeed, so thoroughly was I still labouring under the conviction of the liver being endowed with a glyco-genic function, that I must own, I considered the notion of trying to ascertain if it were free from sugar during life, as almost an absurd one than otherwise. Nevertheless, I determined to see if I could not effect an examination of the organ, as it were, nearer to life than had hitherto been done, by suddenly placing it in a condition to arrest any change that might lead to the production of sugar in the act of, or subsequent to death.

Knowing I had a material to deal with, existing in the liver, which passes with great rapidity into sugar under the influence of ferments, I looked for an agent that might possess the power of preventing this transformation, without destroying the material or its product. Experiments were conducted, apart from the liver, with a solution of amyloid substance, making use of saliva as a ferment. Various agents were tried, to see if I could check the saccharine metamorphosis from taking place without destroying either of the principal bodies concerned. The alkalies were found

to possess this property. A very small quantity of potash, added to a solution of amyloid substance, sufficed to prevent the operation of the transformative influence of saliva. Upon this fact, I was led to perform the experiment of injecting the liver with potash. The alkali having prevented the action of saliva as a ferment, I thought it would exercise a similar effect upon other ferments, and that, by injecting a strong solution of it through the portal vein into the liver as expeditiously after the destruction of life as practicable, I should meet with an organ, as far as amyloid substance and sugar were concerned, in the condition belonging to life,—or, at least, much nearer to life than had hitherto been done,—and thus decide the question which I had started out specially to solve.

Upon the injection of potash being effected, it was in vain I afterwards sought for the accustomed behaviour of the liver. I failed to discover any, or at the most, more than a mere trace, of sugar in the substance of the organ; and I therefore could now satisfactorily account for the difference that had been observed in the state of the blood before and after death. That the potash had not destroyed the sugar, but simply prevented its formation, was shown by performing the injection upon a liver that had been allowed to remain a short time after death to permit the production of sugar to take place. The result, under these circumstances, was, that the presence of sugar was as readily discoverable as if the injection of the alkali had not been employed. A saccharine liver injected with potash gives as copious a reaction as if the injection had not been practised.

To prove, again, that exception could not be taken, by presuming that the liver experimented on was not in a healthy state, and would not have given rise, if it had been left to itself, to a saccharine reaction, the *modus operandi* was modified, and the injection practised instantly after death

on a portion only of the organ. On analysing the two specimens thus obtained, the one gave no reaction with the copper solution, whilst the other occasioned a strong reaction indicative of sugar.

Although it was at the very commencement of my researches on this point that I discovered, that a strong solution of the caustic alkali prevented the production of sugar in the liver after death, yet, it was not until some time afterwards I was aware, that a moderate amount of the carbonated alkali possessed the power so completely as it does of exercising the same effect. In an experiment not long since performed, I found that 200 grains of the carbonate of soda, dissolved in an ounce of water, and injected into the liver of a dog through the portal vein instantly after the destruction of life by pithing, sufficed for the purpose. A couple of lobes happened to escape being properly penetrated by the injection, and in this part of the liver the ordinary *post-mortem* production of sugar took place. In the other portion of the organ, however, no saccharine metamorphosis occurred, the liver structure being found devoid of sugar shortly after the injection; and, even, when examined again on the following day.

Acids act in a similar manner to alkalies in checking the transformation of the amyloid substance by ferments. I have employed a strong solution of citric acid for injection into the liver, and have found it produce the same effect as the solution of potash.

In all these experiments, as may naturally be supposed, every thing depends upon the rapidity with which the injection is effected after the life of the animal has been destroyed. The transformation of the amyloid substance into sugar takes place so speedily after death, that unless the greatest promptitude is exercised an unsuccessful result is sure to be obtained.

Although I believe that these experiments may be relied on as affording accurate information regarding the physiological state of the liver, yet, another and a far more preferable process exists, for enabling us to alight upon the condition naturally belonging to life. Conclusions derived from the employment of chemical agents, such as acids and alkalies, might be regarded with suspicion by some, but the effect produced by the simple influence of a variation of temperature cannot, I think, be open to any likely source of objection. Now, it happens that either of the two extremes of temperature will answer our purpose. Freezing suspends organic chemical changes of every description, and, boiling destroys the ferment without exercising any possible influence upon the amyloid substance or sugar.

For the application of the freezing method, a mixture of ice and salt is employed. The mixture must be allowed to liquefy before being used, its power of rapidly extracting heat being thereby very much enhanced. The object is, to instantly reduce the temperature to a degree, at which the ferment ceases to possess activity. The life of the animal being suddenly destroyed, the abdomen is immediately opened, and a piece of the liver excised as hastily as possible and plunged into the freezing mixture, in which it is afterwards moved about. In the course of a very short time the liver is frozen quite hard. Being removed, it is cut into thin slices, reduced to a pulp in a mortar, and thrown, a little at a time, into a small quantity of water contained in a capsule, which is to be kept thoroughly boiling during the process. If the specimen were allowed to pass through a gradual elevation of temperature, to obtain the decoction for testing, the experiment would be vitiated, as sugar would be formed during the process of preparation. The liquid procured, is a concentrated decoction of the liver. It contains an abundance of amyloid substance, but gives no

indication, or, at the most, a mere trace of indication, of the presence of sugar. With some specimens, where the experiment has been happily conducted throughout, I have not been able to obtain any reaction of sugar; whilst the remainder of the liver, which had not been submitted to the influence of freezing, but tested in the manner it was formerly the custom to do, has given a copious reduction with the copper solution.

To prove that merely a suspension of change has been effected by the influence of the cold, it is only necessary to expose the frozen liver for a short time to a temperature of about 90° or 100°. When afterwards prepared for testing, the most abundant evidence of the existence of sugar is to be obtained.

I need scarcely say, that a liver which has been allowed to become saccharine after death, loses none of its saccharine quality by immersion in a freezing mixture.

I have found the rabbit to be a more favorable animal for this experiment than the dog, because, on account of its smaller size and the thinness of its abdominal parietes, the steps of the operation can be more rapidly carried out; and the liver being thinner, the portion excised is sooner acted upon throughout by the cold. With a thick piece of liver, I have before now noticed, that whilst the outside portion was devoid of sugar, the central part contained it in moderate quantity. It is obvious that, with a thick mass of liver, the centre must escape being influenced for a greater or less period. Indeed, I have seen it, after some minutes' immersion, still quite soft. Whether the animal is at a period of digestion or not makes no difference, that I have perceived, in the result. Usually, in performing the experiment, it is my practice to conduct it a few hours after food has been given.

The result produced by boiling water is similar to that

of a freezing mixture. By coagulating or destroying the materials capable of acting as ferments in the liver, the ordinary *post-mortem* transformation is prevented from occurring. Its action, perhaps, is not quite so perfect as that of freezing, but still, especially with the liver of the rabbit, specimens are easily obtained yielding only the faintest indication of sugar. On testing the decoction, there is no change on boiling with the blue liquid, but after some time, a few particles of red precipitate may subside. It will be easily understood that every degree of elevation of temperature, until the point is attained at which the transformative agent loses its activity, is more and more favorable to the production of sugar; so that, unless an arrival at the requisite degree of heat is comparatively sudden, the object in view will fail to be attained. There should be a good bulk of water, say a quart, into which the piece of liver is plunged, and it should be thoroughly boiling at the time. The piece of liver should not be too large or too thick, and it is desirable to make a few incisions into it, in order that it may be more rapidly acted on throughout. After a minute or two's immersion, all that is necessary for testing is to pound in a mortar, add a little water, and then boil in a capsule to procure a decoction.

As with the injections, so with these experiments upon the influence of temperature, everything depends upon the rapidity with which the required alteration is effected after the destruction of life has been occasioned. At the temperature of the warm-blooded animal, the *post-mortem* transformation of amyloid substance into sugar takes place with such rapidity, that the exercise of the greatest promptitude is required.

In the case of the cold-blooded animal, the circumstances happen to be such, that the application of special expedients

as with the warm-blooded animal, are not needed to procure a representation of the natural living condition of the liver. According to the lowness of temperature, the *post-mortem* change is proportionately retarded. Hence, with ordinary precautions, especially during cold or moderately cool weather, the liver of a cold-blooded animal is readily alighted upon in the state that belongs to life.

The frog is an exceedingly convenient animal to experiment on for this, as for so many other purposes. Now, it did not escape the observation of Bernard in his researches, that according to the temperature of the animal at the time of death, the liver was found to contain sugar or not, after an ordinarily conducted examination. In the 'Comptes Rendus' of the Academy of Sciences, March, 1857, he mentions that lowering the temperature of frogs causes a disappearance of sugar from the liver, whilst a reappearance takes place on afterwards exposing them to heat. He adds, that it is possible to produce this singular alternation of appearance and disappearance of sugar several times, without any food being given, and by acting solely on the phenomena of the circulation through the medium of the temperature. Such is the idea he entertained in explanation of this phenomenon, namely, that it depended on an alteration in the activity of the glyco-genic function, brought about by increasing or diminishing the activity of the circulation.

In experimenting upon frogs, myself, I had obtained results corresponding with those of Bernard, before I was aware he had undertaken his experiments. Frogs in good condition, in which the liver is large, pale-coloured, and exceedingly rich in amyloid substance, were exposed for two hours to an atmosphere heated to 90° Fahr. An examination of the livers in the usual way, afforded a decided indication of the presence of sugar. In other frogs, without exposure to the elevated temperature, the livers similarly examined gave no reaction with the copper solution.

Whilst repeating these experiments, I accidentally met with a result for which I was formerly at a loss to account. Some frogs that had been exposed to heat happened to be placed aside in my laboratory for a quarter of an hour before they were killed. Their livers, being examined, yielded scarcely an indication of a trace of sugar. Time had been given for the temperature to fall; and, from what has preceded, it will now be seen, that the absence or presence of sugar in these experiments depends upon the influence that a high or low temperature has been shown to exercise over the *post-mortem* change that occurs in the liver.

The oyster and the mussel are animals which likewise serve well to illustrate the true state of the liver in regard to sugar formation. In both, the liver is charged with a very large quantity of amyloid substance, but is entirely devoid of sugar when the animal is taken for examination in a fresh and healthy condition. Should the animals, however, have been allowed to die, or been kept for some time out of water, sugar is then discoverable in abundance. The same is the result when the livers are removed and moderately heated a short time before the examination is made.

In the mussel (*Mytilus edulis*), besides the amyloid substance contained in the liver, the mantle is exceedingly rich in an identical material under a good conditioned state of the animal. When the animal is poor, as after spawning, the mantle is thin, transparent, watery, and comparatively devoid of amyloid substance; but at other times, it is thick and opaquely white or yellowish, and, in this state, is highly charged with the amyloid substance, which therefore seems to form a store to be drawn upon when required by the economy. No sugar can be detected in the mantle at the time of death; and, what is more, there is scarcely a proneness to the production of sugar after death, unless a ferment,

such as saliva, be added, when a copious formation of sugar is the result.

The liver of a warm-blooded animal, even, may be placed at the time of death in nearly the same position as that of a cold-blooded one, by the adoption of certain measures during life. After division of the spinal cord as high up towards the brain as is consistent with the persistence of life—that is, just below the origin of the phrenic nerves—the temperature of the body rapidly falls, when the animal is placed in a cool situation. Bernard, again, noticed that under these circumstances the liver was found devoid of sugar when examined immediately after death, but became strongly saccharine afterwards. He endeavoured to account for this phenomenon under his glyco-genic theory; but the different reasons assigned, show the difficulty he was under in giving a satisfactory explanation of it. The fact itself is a most striking one, and has been frequently corroborated in my laboratory. It fully agrees with all the experiments I have been mentioning, and receives from them, I consider, a satisfactory explanation. The low degree of temperature at the time of death diminishes the rapidity of the *post-mortem* formation of sugar, which, being thus retarded, is easily recognised in its proper light. In one case, three hours and a half after the operation of division of the cord had been effected on a rabbit, I noticed that the temperature of the rectum was only 67°.

Maintaining artificially the temperature of the animal, by exposure to a heated atmosphere, after division of the spinal cord, essentially alters the result. Thus, a rabbit in which the spinal cord had been divided just below the phrenics, was placed in an atmosphere where the thermometer stood at 88°. In three hours' time, when its life was destroyed, the temperature of its rectum was 104°. The liver behaved precisely as if the animal had been suddenly killed whilst in a

healthy state; sugar being found, after an ordinarily conducted examination, to its usual extent. The temperature was conducive, as under ordinary circumstances in the warm-blooded animal, to the almost instantaneous production of sugar after death.

The same result may be obtained, by oiling the coat of a rabbit and thereby favouring the rapid escape of heat from its body, as by division of the spinal cord. In a warm atmosphere a rabbit will outlive such an operation; but, exposed to a cold atmosphere, the radiation of heat, from the destruction of the non-conducting properties of the coat, being greater than the power of generation, the temperature more or less rapidly declines until it reaches a point that is inconsistent with the further continuance of life. When the temperature of the body has fallen to about 65°, life becomes thoroughly extinct. On the destruction of life before the animal has reached a moribund state; when, in fact, the temperature has descended to between 70° and 80°, the liver is easily alighted upon in its physiological state, and becomes strongly saccharine afterwards.

From a consideration of the facts that have been brought forward, which, in every respect, corroborate each other, and which, I may mention, stand uncontroverted by any result I have yet encountered, I think we are unavoidably led to deprive the liver of that sugar-forming function with which, in recent days, it has been endowed by physiologists. Our position in reality stands thus. The conclusions upon which our former notions were based, were drawn from results obtainable after death. But, it now becomes apparent that we are no longer justified in regarding these results as indicating the condition that belongs actually to life. It is not, that Bernard's observations are incorrectly recorded, or his experiments inexact; but, that fallacious inferences, as shown by more extended investigation, have

been drawn from these experiments and their results. The views I have advanced are perfectly compatible, not with our former conclusions it is true, but with the experiments upon which those conclusions were founded. From an ordinary examination of the liver and the blood of the right side of the heart after death, we obtain reactions that infallibly indicate a large impregnation of sugar. The deduction from this hitherto has been, that the sugar existed there naturally during life. This deduction, however, although it has appeared to our minds exceedingly plausible—so plausible, indeed, that no one before has been led to question it; yet, confined to such experiments, it is obviously gratuitous. All that can be strictly or logically inferred from the result of such an examination is, that the liver and the right-ventricular blood are strongly saccharine *after death;* to show that this is likewise the condition *during life* requires another mode of experimenting. And, notwithstanding, as it is fair to confess, nothing was to have been reasonably expected beyond a ratification of our views, yet, on actually prosecuting the inquiry, it turns out that we can no longer overstep the strict letter of interpretation belonging to the original experiments.

Briefly to recapitulate, the following are the conclusions that arise out of the facts that have as yet been mentioned in reference to glyco-genesis:—

Evidence has been given that the so-called glyco-genic matter is formed in the liver from sugar as one of its sources. Hence, if sugar were its natural destination, the process would be sugar into glycogène, and glycogène back again into sugar—a repetition that certainly appears, to say the least of it, extraordinary and improbable, as a designed occurrence of nature.

The blood, under normal circumstances, only contains

more or less evident traces of sugar; and that belonging to different parts of the system does not present, as far as I can discover, after adopting the necessary precautions in the examination, any appreciable variation in constitution. In some most carefully conducted examinations, I was unable to distinguish, by any difference in behaviour, the blood of the portal vein from that of the right side of the heart.

The liver itself is found free from sugar, or, at the most, is only impregnated with it, to the minutest extent, when treated so as to come down upon it in a condition the nearest possible to that of life.

Now, reflecting upon what I have described to be the natural state of the blood and liver during life, and, upon the circumstance that the amyloid substance possesses the strong tendency it does to transformation into sugar, the question naturally forces itself upon us for consideration—how is it that the production of sugar, which occurs so rapidly after death, is prevented from likewise taking place during life? I must candidly admit, that all I can do at present is to reiterate, that such is the fact, without attempting an explanation. I can only say, it is a matter susceptible of observation, that the amyloid substance is endowed with the power to resist, whilst located in the tissue of the living and healthy liver, transformation into sugar, and that, with the destruction of life, such power of resistance ceases to exist. When the forces of chemistry are in unopposed operation, as after death, the tendency of the amyloid substance to undergo a saccharine metamorphosis, is allowed to take effect. Under normal circumstances during life, however, this mode of transformation is held in check by some force or condition belonging to the living and healthy liver. There is the material which is so susceptible of undergoing

change into sugar, and there is, also, the ferment or agent for exciting it. Both are present in the liver, and yet the process is observed to escape being carried out, except to an almost infinitesimal extent.

I may mention that in the course of my experience two instances have fallen under my observation, where the amyloid substance has remained in the liver without undergoing its ordinary *post-mortem* transformation into sugar. The first, was the liver of a cod-fish, which fell into my hands in January 1860. The organ was most richly charged with amyloid substance, but gave so slight an indication of the presence of sugar, that, on boiling the decoction with the copper solution, there was not the slightest perceptible reaction to be seen, and even, after placing the test tube aside for some time, an amount of deposit that was only just distinctly appreciable was all that was found to have subsided. On the following day the liver behaved in identically the same manner. And, what was more surprising, a specimen of it exposed for three hours to a temperature of about 90° only contained just an evident trace of sugar. The addition of a ferment in the form of saliva to the decoction instantly occasioned the production of a copious amount of sugar. The decoction, indeed, was so highly charged with amyloid substance as to be quite opaque like milk. In the other instance, which occurred to me quite recently, the liver was derived from a rabbit. The animal was an old buck rabbit in an exceedingly healthy and robust condition. It had been feeding on carrots shortly before being killed. A decoction of the liver made in the ordinary way a short time after death, although opaque like milk from the abundant presence of amyloid substance, gave no reaction indicative of sugar. After the liver had been exposed, also, for several minutes to a moderately elevated temperature there was only just a trace of saccharine reaction discoverable. Treating

the decoction with a little saliva immediately occasioned a large production of sugar. The only interpretation that I can give to these results is, by supposing that there did not exist the usual ferment present in the liver; or else, being there, that its action was checked by the presence of some other agent. But, whatever the interpretation, the results themselves cannot be looked upon as otherwise than of an exceedingly interesting and important description. Care was taken in each instance, by making several examinations, to secure that no chance of error had crept in. It may be added, as a matter of general experience, that I have found the *post-mortem* production of sugar take place much more slowly in the good-conditioned rabbit than it does in the dog, there being at the same time a much larger proportion of amyloid substance present in the former than in the latter.

Although I cannot attempt to offer an explanation of why the amyloid substance escapes as it does transformation into sugar during life, whilst the effect takes place with such rapidity after death, yet the fact remains the same, and we are not altogether unprepared for it, as we have long been familiar with an occurrence—the phenomenon of coagulation—which seems to me may be considered as occupying a parallel position. Whilst circulating in the vessels of the living body, fibrine exists in a fluid state. Immediately, however, that blood is withdrawn, this fibrine, with an astonishing rapidity, assumes the solid form. It is the property of fibrine to have a strong tendency to coagulate. As long as it remains under natural circumstances in the living body, this tendency is held in check, but immediately it escapes from the influences thus naturally controlling it, its coagulating property comes into play, and a solidification is the result. Even here, although the phenomenon has been so long

familiar to us, it has not yet received what can be admitted as a satisfactory and full explanation. There is, then, nothing particularly wonderful, nothing unparalleled, in the position of the amyloid substance before and after death—namely, as to its resisting during life the exercise of its chemical tendency to transform into sugar, and a transformation taking place immediately the controlling influence—whatever this may be—existing during life is removed. No one, for a moment, would attempt to deny the fluid existence of fibrine during life, because he could not satisfactorily account for what held in check the solidification that takes place after death; and similarly, I contend, no one would be justified in ignoring the capability of the amyloid substance to resist during life the saccharine metamorphosis, because he could not explain of what nature this power of resistance consisted.

Another important link that, it must be confessed, is yet deficient in this inquiry is the discovery of the body into which the amyloid substance is designed for physiological transformation. I will merely mention in connection with this point, that I have grounds for believing, that the product of the normal transformation of the amyloid substance, passes down through the biliary passages into the intestine; and, that I have been for some time past actively engaged in tracing out the connection of the liver with the production of fat—a statement that will serve to throw a hint upon the direction in which my views are tending with respect to the purpose of the amyloid substance.

That there *must* be some other mode of transformation, to which amyloid substance is susceptible in the liver, besides conversion into sugar, is proved by the results I have obtained after the introduction of the carbonate of soda into the portal system during life. A solution of carbonate of

soda has been injected into one of the branches of the portal vein, and in a few minutes even, has created a total disappearance of amyloid substance from the liver, without any evidence of the production of sugar.

Presuming, for the sake of argument, that the amyloid substance had disappeared, under the influence of this injection of carbonate of soda into the portal system during life, by transformation into sugar, it is easy to determine how much of this principle should have been produced. I, some time since, made some analyses to ascertain the relation that exists, as to loss and gain, in the transformation of amyloid substance into sugar, in the liver after death—to ascertain, in fact, how much sugar is formed for the amyloid substance that disappears. In the first three experiments, a part of the liver was instantly after death thrown into a freezing mixture, whilst the other part was allowed to remain in the animal for some minutes. An analysis of each specimen being made, gave me the quantity of amyloid substance lost in the portion where the temperature was kept up, and likewise the amount of sugar that was formed. In the fourth, the liver was simply removed from the animal and examined at once, and then again in twenty-four hours' time.

	Amount of amyloid substance lost. per cent.	Amount of sugar gained. per cent.	Relation of gain of sugar to loss of amyloid substance.
No. 1	2·20	1·57	1 to 1·40.
No. 2	7·04	4·25	1 to 1·65.
No. 3	3·12	2·05	1 to 1·52.
No. 4	1·82	1·12	1 to 1·62.

Looking at the medium of these results, we get for the production of one part of sugar a loss of 1·54 of amyloid substance. Now, taking an ordinary-sized liver of the dog, say seven and a half ounces in weight, containing an average amount of amyloid substance for an animal diet,

say seven per cent., and reckoning in round numbers that a loss of one and a half part of amyloid substance is accompanied with a production of one part of sugar, then upwards of 150 grains of sugar would have to be accounted for, if under the influence of the carbonate of soda the rapid disappearance of amyloid substance were through its undergoing a saccharine metamorphosis. Carbonate of soda having no power, as far as I can discover by experiment, of directly leading to the destruction of sugar in the system; and, there being no possibility of failing to detect such an amount as that above mentioned, it is evident that there must exist some other mode of transformation for the amyloid substance in the animal system besides into sugar.

In various parts of the body, especially in early life, there is to be found a material which has characters indentical with the amyloid substance of the liver. Now, if this amyloid substance give rise to sugar in one situation, it may be reasonably asked, why should it not do so in others? and, why should there not be in the muscles, and the cells of the cutaneous, intestinal, respiratory, and genito-urinary surfaces of the fœtus, and in the muscles and lungs of the adult, a glyco-genic function?

I imagine that, under the existence of natural circumstances, the circulation takes place through the liver without disturbing the amyloid substance contained in the hepatic cells, further than, perhaps, just a trace of it escaping into the bloodvessels, and thus accounting for the trace of sugar constantly met with in the different parts of the circulatory system. Amyloid substance cannot be mixed with the blood without being transformed into sugar. The blood, whether circulating in the body, or experimented with out of the body, acts, like saliva, as an energetic ferment, in converting amyloid substance into sugar. The injection

of amyloid substance into the circulation of the living animal occasions a saccharine state of the blood; and, if enough be employed, the urine may be rendered strongly impregnated with sugar. Hence, it is only necessary, from some circumstance or other, for the amyloid substance to pass from the liver cells into the blood, and a saccharine state of this fluid is the immediate result.

It can hardly be conceived necessary, looking to the circumstances of the case, that the liver should be reputed to enjoy a special sugar-forming function, to account for the presence of that minute quantity of sugar met with in the circulation as a natural occurrence. Such minute quantity of sugar may, indeed, be reasonably regarded as simply resulting from the operation of the chemical tendency which the amyloid substance happens, as it were, accidentally to possess, which chemical tendency is not only independent of, but even opposed to, the physiological performance of the requirements to be carried out by the liver.

It may be mentioned that the circumstance has not been overlooked, that although the quantity of sugar found in the blood of the right side of the heart is extremely small, yet, supposing this small quantity to have just wholly escaped from the liver, the amount of sugar reaching the circulation during the course of the twelve or twenty-four hours would form an important item of consideration. Such, however, is not what has taken place. The sugar, small in quantity as it is, in right-ventricular blood, has not just been poured into the circulation from the liver. It must be remembered, that the blood flowing towards the heart, through the general systemic veins, contains sugar, and, as far as I can learn from the reaction of our tests, to about the same extent as the cardiac blood itself. So that, sugar having once arrived in the circulation, circulates over and over again, and it would be in the highest degree

erroneous to calculate, from the amount found in the heart and the quantity of blood passing through the liver, the extent of sugar that enters in a given time. Even, as I have stated, the blood that is reaching the liver is charged with sugar, and, as I have already mentioned, in some experiments on animal feeders, where the greatest care was exercised—where every precaution was taken to obtain the specimens in the state naturally belonging to life, there was no appreciable difference of reaction discoverable between the portal and cardiac blood.

I have sought for, but have been unable to obtain, since I became aware of the fallacy to which our former mode of experimenting was exposed, evidence of sugar undergoing destruction, to any extent, in the circulation.* The result of experimental research in my hands on this point, as it is found it must now be conducted, decidedly is, that if sugar escape from the liver, it is to be detected alike in every part of the circulation. It is possible, however, and even, I think, probable that, without being susceptible of appreciation by our tests, there may be a slight amount of transformation of sugar into lactic acid taking place in the

* It is right to mention that I once entertained a different opinion. Participating in the conviction that formerly prevailed, I did not suspect the fallacy to which our former method of experimenting was exposed. I believed that the blood between the liver and lungs was as strongly saccharine whilst circulating during life, as it is found to be when collected a short time after death. I did not know that an interval of only a few minutes could create so marked a difference in the composition of the blood. And, when I met with blood collected instantly after death and injected through the vessels of an artificially inflated lung, presenting but a slightly saccharine character, I thought that sugar had been destroyed by some change induced through the influence of artificial respiration. It was by continuing my investigations upon this subject that I was led to recognise the error under which we had been labouring. To enable me to obtain blood more advantageously circumstanced for injection, I adopted the plan of procuring it by means of a catheter passed into the right side of the heart during life. I need not here enter upon the unexpected results that were observed, the train of circumstances that followed having already received attention in a former part of this work.

blood as it is circulating through the different parts of the system; for, if saccharine blood be allowed to decompose, the sugar, when decomposition has fully set in, gradually disappears, and the blood becomes acid, which is the reverse of what takes place when a specimen has been taken, that is free from sugar. The changes of decomposition, I apprehend, have excited the lactic acid fermentation, precisely, as in the case of milk that has turned sour. Now, I see no reason, although I can bring forward no actual proof in support of it, why the changes going on in the blood, during its circuit through the body, may not act in the same way as the changes of decomposition, and occasion a slow production of lactic acid, from the trace of sugar, that I have mentioned as being found, in every part of the circulation. In this way would be accounted for, the production of the lactic acid that is required for digestion. Whatever, however, may occur of this kind, must be of an exceedingly trifling nature, for the disappearance of sugar is not to be detected in any particular portion of the system by a difference in behaviour of the blood.

Sugar cannot exist to any notable extent in the blood without being immediately drawn upon and separated by the kidneys, so as to become recognisably present in the urine. This being the case, the process of sugar-formation would prove a most fruitless one; and, apart from other considerations, it is not, I think, to be conceded as probable, that the liver should produce sugar for the kidneys afterwards to get rid of.

Reasoning upon what has preceded, I arrived at the conclusion that sugar ought to be found to a slight extent in healthy urine; and in one of the Lettsomian Lectures, published in the 'Lancet,' December 22nd, 1860, I expressed myself in the following terms upon the subject:—"I should say even, that the trace of sugar which is natural

"to the blood throughout the body, is constantly being "drawn upon by the kidney; but that the amount for re- "moval is so small, that it is not susceptible of detection in "the urine. Did we possess more perfect means of detec- "tion than we do, I imagine a minute trace of sugar would "be reckoned as a normal constituent of the urine. I am "speaking now a little problematically, but what I have "said accords with a principle that is being forcibly "recognised in analytical chemistry—namely, that the "minuteness of diffusion of materials through the different "constituents of the universe exceeds or surpasses our "powers of detection."

It was not until after the above was in type and in course of publication that I became aware, through Dr. Bence Jones's paper, read at the Chemical Society, December 20th, 1860, and published in its 'Tranactions' for April, 1861, that sugar had been proved experimentally by Brücke to be a constituent of healthy urine. Dr. Bence Jones's exceedingly complete analytical researches on this point leave nothing to be desired. He confirms Brücke's statement by evidence so striking as to place the matter beyond all dispute. To me, this result is a most interesting and important one. I consider it affords one of the strongest arguments that could be adduced against sugar being formed and poured into the circulation as was formerly supposed to be the case. If so minute a quantity of sugar as that which I have shown, naturally reaches the general circulation, passes off to a recognisable extent in the urine, what, it may be asked, might be reasonably expected to be the condition if a large quantity instead were continually escaping from the liver? I have no hesitation in saying, if such were the case, that we should all be labouring under Diabetes Mellitus.

On giving attention to a physical character which the

amyloid substance possesses, the explanation is clear, how, under natural circumstances, it resists, as it does, transudation into the circulating current, where, being transformed into sugar, it would in great part, if not entirely, escape with the urine, and prove useless. It is, indeed, most interesting to notice how the amyloid substance, in the feeble property of diffusibility it enjoys, presents us with an exemplification of the harmonious adaptations that exist in the exercise of the operations of nature.

Looking at the amyloid substance as it is contained in the hepatic cells, and reflecting on the close proximity that the blood must hold to it, in circulating through the liver, it may be reasonably inquired, how it is that it does not pass, as it might be expected to do, by diffusion into the blood-vessels—a circumstance, as has been observed, that would conduce to render the blood extensively saccharine. Now, it happens, as I have discovered, and as can be easily verified by experiment, that amyloid substance is diametrically opposed to sugar as regards its susceptibility of diffusion. If sugar were contained in the liver cells, the circulation could not take place through the organ without largely carrying it away, on account of its being so readily diffusible through membranous septa. With amyloid substance, on the other hand, there is a resistance to traverse animal membranes. A solution of sugar and amyloid substance being placed together on one side of an animal membrane in an endosmotic apparatus, the sugar may be observed to diffuse itself and leave the amyloid substance behind. I have even found, under the pressure of a column of fluid six feet high, that amyloid substance has resisted passage through the pores of an ordinary piece of bladder. It is on account of this low diffusibility, I presume, that we experience the difficulty we do in filtering its aqueous solution. The coarse pores of an ordinary

filter seem, indeed, to offer considerable obstruction to the passage of this principle. That the amyloid substance *can* pass, however, under certain circumstances, through a thin membranous septum is sufficiently proved by the fact, that on injecting a stream of water through the vessels of the liver, the amyloid substance is gradually washed out. Under such an operation, the liver becomes extraordinarily swollen and œdematous.

Albumen is another material which, like amyloid substance, enjoys a low diffusibility. When a solution of it is submitted to experiment, it does not pass through a thick membrane, such as a piece of bladder, except under the influence of pressure. With a column of fluid, three feet high, the albumen passes, but not at all when no pressure is employed. This property of albumen is doubtless connected with its retention in the bloodvessels. It is not intended that a material so necessary as a pabulum for the nutrition of the animal tissues, should have a tendency, like urea and sugar, to escape from the blood by virtue of the physical property of diffusibility. For a corresponding reason, I apprehend, the amyloid substance agrees in this respect with albumen, and is not endowed with a tendency to diffuse, because it is not naturally designed for escape, as such, from the hepatic cells into the bloodvessels.

The amyloid substance, resisting as it does, passage through animal membranes, and having therefore no tendency to escape from the hepatic cells, prevents the system from being saturated with sugar; for, the effect of amyloid substance reaching the circulation, is an immediate transformation, through the catalytic agency of the blood, into sugar. Under the ordinary course of events, the amyloid substance can neither be transformed in the liver into sugar nor escape into the bloodvessels, to more than a scarcely appreciable extent, because of the state which the blood is found

naturally to present. But, there are several ways in which, by experiment, such a deviation from the ordinary course of events may be occasioned as to determine a flow of sugar into the blood, that shall give to the urine a strongly saccharine impregnation. This production artificially of a diabetic state of the urine may be conveniently examined separately under the following heads:

1. The Circulation.
2. The Blood.
3. The Nervous System.

State of the Circulation.—Under normal states of the circulation, the blood passes through the liver without, certainly in any material degree, disturbing the amyloid substance contained in the hepatic cells. The amyloid substance, as has been mentioned, offers resistance to traversing animal membranes, and hence its retention in the liver, notwithstanding its close relation to a circulating current. Sugar, with its strong tendency to diffuse, could not possibly be retained under similar circumstances. That the amyloid substance *can* be made, however, to escape from the liver tissue into the bloodvessels, is proved by the effect, already referred to, of injecting a stream of water through the organ after death. The liver swells up and becomes exceedingly œdematous, and the water which has passed through is more or less charged with amyloid substance. Suppose such an escape of amyloid substance to occur during life, the blood will be thereby rendered saccharine, and so a saccharine state of the urine induced.

Now, may it not be, that we have here an illustration of the manner in which, saccharine urine is really occasioned in some instances? Violent muscular efforts, by which the liver is compressed, and obstruction to the breathing, by which the circulation is interfered with and

congestion produced, will, I know, as a matter of experience, occasion a highly saccharine state of the blood. In the case of the latter, by keeping up the respiratory disturbance—by simply limiting the supply of air short of producing asphyxia for an hour, I have rendered the urine of an animal strongly saccharine: the urine, tested before the experiment was found perfectly devoid of sugar; whilst, after the experiment, it gave a copious orange-yellow reduction with the copper solution. Both disturbances, as far as the liver is concerned, are of a physical character; and is it not purely to physical circumstances that the unnatural condition observed is attributable? When the liver is compressed, a favourable condition is established for promoting the escape of its amyloid substance; and when it is congested, the hepatic cells being submitted to compression, such condition, may, I conceive, in like manner, be favourable to a transudation of amyloid substance, and its admixture with the blood.

In the human subject, saccharine urine has been noticed in cases attended with obstruction to the proper flow of blood through the system. For example, it has been observed in whooping-cough, coma, and pneumonia; and I look upon it here as entirely referable to the effect of the altered physical conditions acting upon the liver. In whooping-cough, the protracted paroxysms of coughing, not only occasion a great amount of venous congestion, as is indicated by the countenance of the patient, but the liver must, at the same time, be compressed externally by the violent action of the abdominal muscles. In coma with stertorous breathing, the impediment to the circulation of blood through the lungs produces general congestion of the system, and in this, the vessels of the liver must participate. I may mention a case of this kind, where the urine was found quite strongly saccharine. A man, one of the carpenters of

Guy's Hospital, suddenly fell down in the street in an apoplectic fit. As there was great lividity of the face, with slow, stertorous breathing, I suggested, about eighteen hours after the seizure had taken place, that the urine should be tested for sugar. It was highly albuminous, and gave, after the albumen had been coagulated and separated, a copious reaction with the copper solution. There was no evidence that this person had saccharine urine previous to the occurrence of his fit. In pneumonia, where a large portion of the lung is rapidly involved, the patient is liable to suffer from great dyspnœa; and, through the freedom of the circulation being interfered with, saccharine urine may be occasioned at the commencement of the disease.

After division of the pneumogastric nerves in the lower animals, I have sometimes observed the production of even strongly saccharine urine. This does not depend upon any direct influence that division of the nerves has upon the liver, but upon the operation effecting such a diminution in the activity of the respiratory process, as to occasion a serious impediment to the natural flow of blood through the system. It is only where this has prominently been the case that the sugar has been recognised in the urine.

It is in this way, that I believe the administration of chloroform acts, when it determines the presence of sugar in the urine. It is true, chloroform itself, produces some amount of reaction with the copper test; but the precipitate comes down in a rather different form, and, from other considerations, it would seem to be to sugar, that a reaction obtained from urine after the administration of chloroform, is principally, if not entirely due.

This statement, that the inhalation of chloroform may produce saccharine urine, is not a new one. It was long since mentioned by Reynoso and others that, after the inhalation of ether and chloroform, more or less sugar might be found

in the urine. I scarcely thought, however, until recently, that the effect was so constant and so marked as it appears to be. A result was one day obtained in my laboratory, which induced me to seek for further experience on the point. Chloroform was being administered to an animal, and, whilst the urine which escaped at the commencement, gave no reaction with the copper solution, that withdrawn about twelve minutes later, produced an orange-yellow reduction. The animal had been simply kept under the influence of chloroform during this time, without any experiment being performed.

The extent of time during which an animal is kept under the influence of chloroform, seems to have little or nothing to do with the amount of sugar to be met with in the urine. In an instance where anæsthesia was kept up for between three and four hours, there was only at the end of this time a slight amount of sugar in the urine. It has appeared to me, from what I have seen, that the effect has been dependent on the extent of struggling and congestion attendant on the exhibition of the anæsthetic, rather than on anything else.

Upon noticing the above-mentioned occurrence, where the urine became so rapidly and so strongly saccharine, I thought it would be interesting to be able to speak personally of the effect of chloroform in this respect, upon the human subject, from an examination of the urine in a number of cases taken indiscriminately from the operating theatre of Guy's Hospital. My pupil, Mr. Lamb, who has often rendered me much assistance in my laboratory, kindly undertook to collect for me twenty cases. The following is a list of them, with the results. They were procured during the summer of 1861. The urine examined was the last passed before, and the first passed after, the inhalation of chloroform.

Examination of the urine for sugar in twenty cases, from the operating theatre of Guy's Hospital, before and after the inhalation of chloroform.

No.	Name.	Age.	Nature of operation for which chloroform administered.	Urine tested with the copper solution.	
				Before chloroform.	After chloroform.
1	Fred. B—	45	Removal of tumour on leg	No reaction	Slight reaction.
2	Thos. W—	35	Amputation of penis	No reaction	Moderate reaction.
3	Hannah W—	44	Removal of tumour of breast	No reaction	Moderate reaction.
4	Richard P—	6	Removal of diseased bone	Slight reaction	Considerable reaction.
5	Eliz. C—	38	Removal of tumour of back	Very slight reaction	Considerable reaction.
6	Emma T—	12	Amputation of thigh	No reaction	Considerable reaction.
7	Louisa B—	25	Amputation of thigh	No reaction	Considerable reaction.
8	Jane M—	28	Amputation of forearm	No reaction	Considerable reaction.
9	George J—	27	Removal of necrosed bone	No reaction	Strong reaction.
10	John H—	33	Amputation of foot	Slight reaction	Slight reaction.
11	Ann W—	33	Amputation of thigh	No reaction	Considerable reaction.
12	Ellen E—	8	Amputation of foot	No reaction	Slight reaction.
13	Emma H—	14	Operation for club foot	No reaction	Considerable reaction.
14	George W—	$2\frac{3}{4}$	Removal of necrosed bone	No reaction	Considerable reaction.
15	Joseph S—	66	Amputation of thigh	No reaction	Strongish reaction.
16	Jane W—	36	Removal of necrosed bone	No reaction	Trace of reaction.
17	Louisa K—	24	Removal of tumour of breast	Slight reaction	Moderate reaction.
18	Eliz. E—	31	Examination for stone	No reaction	Considerable reaction.
19	Fred. B—	45	Amputation of thigh	No reaction	Considerable reaction.
20	Charles T—	41	Amputation of foot	No reaction	Considerable reaction.

From this table it appears that out of twenty cases in which chloroform was administered for anæsthetic purposes, to patients undergoing various operations, in only one (No. 10) was there no effect upon the urine discoverable. In most, the reaction afforded after the inhalation was a considerable one; and in some, quite a strong one. In four of the cases, it will be observed, the urine collected, before the administration of the chloroform, gave a slight indication of the presence of sugar. It is no uncommon occurrence for the urine of patients to behave in this way, and give more or less reaction with the copper solution, without being submitted to concentration or any special process of preparation. Mr. Long and Mr. Gibson, whilst acting as clinical reporters at Guy's Hospital for Dr. Rees and Dr. Gull, tested the urine of a large number of patients labouring under various complaints. They found in several cases, and particularly in cases of phthisis, that the copper solution afforded a more or less strongly marked reaction. In some of the results that were shown to me, quite a fair amount of sub-oxide deposit had been thrown down.

State of the Blood.—I apprehend that a certain state of the blood circulating through the liver is required for the proper performance of its functional operations, and that certain unnatural states of it may cause a retrogression of the amyloid substance into sugar. A supply of portal blood seems necessary to hold in check the saccharine metamorphosis; for, strikingly enough, a ligature applied to the portal vein causes the blood circulating through the system to present a strongly saccharine character.

Let us pause for a moment, and look at this result in reference to the glyco-genic theory. It will hardly, I think, be found to be reconcilable with the admission of the doc-

trines that have given to the liver a sugar-forming function. If there were the constant production and flow of sugar from the liver through the hepatic veins, as is implied under the glyco-genic theory, on interrupting the principal stream of blood through the organ, it would certainly be only reasonable to expect that less sugar should escape, and less sugar be found in the circulatory system. Yet, as the result of actual experiment, it would appear, that precisely the reverse happens to be the case. Notwithstanding the current is so reduced, which is supposed to be carrying sugar into the general circulation, as a part of a particular functional operation; the amount of sugar now reaching the circulating fluid is actually very greatly increased. Apparently, in accordance with the facts of the case, it may be said, that the ligature of the portal vein places the liver in an unnatural condition: that the functional working of the organ does not proceed, with a supply only of arterial blood, in the manner it does, when there is also the stream of portal blood to exercise its influence. And, as the result of this, the amyloid substance descends into sugar, like it does under many other unnatural states of the liver.

Ligature of the hepatic artery as well as the portal vein, causes the liver to become strongly impregnated with sugar during life, as is the case with ligature of the portal vein only; but the blood belonging to the general circulation remains uninfluenced. The flow of blood through the liver being entirely prevented, no opportunity is afforded for the passage of the sugar into the system.

The introduction of an acid into the system so as to alter the natural quality of the blood, I have found to produce saccharine urine. This effect has followed the injection of phosphoric acid into the general venous system, and also its introduction into the intestinal canal. I conceive it acts

by altering the state of the blood, and rendering it unsuited for contributing towards a natural performance of the operations carried on in the liver.

Dr. Harley, some years since, found that the injection of small quantities of ether and ammonia into the portal system determined the presence of a certain amount of sugar in the urine. M. Lecomte, also, has noticed that saccharine urine may be occasioned by the administration of small doses of the nitrate of uranium. I take it, again, in these cases, that the functional working of the liver is disturbed through the medium of the fluid traversing its vessels.

State of the Nervous System.—By depriving the liver of the force it naturally derives from the nervous system, the chemical tendency of its amyloid substance to transform into sugar ceases to be held in check; and thus a saccharine state of the whole system may be rapidly induced. A diabetic condition of the urine may, indeed, be occasioned by simply destroying life and keeping up the circulation artificially for a time. The life of an animal, for example, is destroyed; the chemical tendency of the amyloid substance is allowed to come into play. Sugar is immediately formed in the liver, and diffuses itself into the blood-vessels. It is only now necessary for the circulation to be maintained, and a saccharine state of the urine will be produced. The circulation is easily kept up after death by the performance of artificial respiration; and where this has been carried out, it has been found, as above mentioned, that the sugar formed in the liver, as the result of a *post-mortem* occurrence, has been carried to the kidney and eliminated with the urine. One hour's performance of artificial respiration has sufficed to give a fair quantity of urine presenting a highly saccharine character.

In the result that has just been referred to, life has been

supposed to have been removed by the destruction of the *medulla oblongata*. The experiment, however, is attended with the same effect, when death is occasioned by poisoning with agents capable of acting rapidly through the cerebro-spinal system. Bernard has stated, and my own experience confirms the accuracy of the statement, that after killing with the woorali poison, if the circulation be kept up by the performance of artificial respiration, the urine is rendered strongly saccharine. I have also found, that in substituting strychnine for the woorali, the effect is identically the same.

Some time since, the striking announcement was made that artificial diabetes could be produced by a slight puncture made in a certain part of the *medulla oblongata*. Bernard's statement was soon confirmed by others, and this interesting result was looked upon as adding another link to the evidence in support of the glyco-genic theory. The spot that it is necessary to reach, to produce this effect is an exceedingly limited one, but when the experiment has been successfully performed, the urine in about an hour, or even in a less time than this, is rendered strongly saccharine. This saccharine quality, however, is only of a temporary duration, never lasting beyond, at the outside, the extent of a few days. It was considered by Bernard, that the effect resulted from an exaltation of the glyco-genic function, through an irritation of the centre belonging to the pneumogastrics. From what has preceded, it will be evident, that I cannot admit the accuracy of such a conclusion. The instrument for making the puncture is provided with a couple of cutting edged shoulders, and produces an incision into the nervous substance, which I regard as creating a loss, instead of an exaltation, of nervous influence. I look upon this operation as acting in the same way as complete division of the *medulla oblongata*.

In the latter, however, other processes besides those belonging to the liver, are interfered with, and artificial means are required for keeping up the circulation to obtain a diabetic state of the urine.

Although division of the *medulla oblongata* leads to the production of saccharine urine when the circulation is kept up artificially, yet such is not the case with division of the spinal cord. Saccharine urine is certainly not produced by division of the spinal cord below the origin of the phrenic nerves, after which, the animal is able to breathe without assistance. And, when the cord even has been divided as high up in the canal as between the second and third cervical vertebræ—an operation, after which, like after division of the *medulla oblongata*, the employment of artificial respiration is required to maintain the circulation—no sugar, according to my experience, has been discoverable in the urine.

Again, the brain does not seem to exercise the same influence over the state of the liver as the *medulla oblongata*. It is true, I am not enabled to speak in quite such positive terms with respect to this organ; but my experiments induce me to believe that its functional influence may be removed without placing the liver in the condition noticeable after actual death, or after division of the *medulla oblongata*. In operating on the brain, circumstances are apt to arise that may complicate the result. Where, however, there has been the least complication, I have observed that the urine has remained devoid of sugar, after a complete separation of the brain proper from the other parts of the nervous centre, by division of the *crura cerebri* just in front of the *pons varolii*.

Looking to what I have just stated, the natural inference seems to me to be, that the cerebro-spinal system gives to the liver during life, a force or a condition, which it does

not possess after death; and, that the part of the cerebro-spinal system particularly connected with this purpose is the *medulla oblongata.*

Regarding the *medulla oblongata,* then, as a centre, either directly presiding over the functional activity of the liver—giving it a force capable of holding in check the chemical tendency of its amyloid substance to transform into sugar—or indirectly affecting it, by altering, through the medium of another or other organs, the condition of the blood going to it; I determined to look for the channel through which its influence might be transmitted downwards. It was, whilst working with such an object before me, that I was led to discover, that a strongly diabetic state of the urine might be rapidly occasioned by injuring or dividing certain portions of the sympathetic nervous system.

Supposing a force to start from the *medulla oblongata* for the purpose I have mentioned, I premised that its transit to the liver could not be, at least, exclusively through either the spinal cord or the pneumogastrics, as division of each had been frequently practised without any diabetic effect having been produced. But the result of dividing both cord and pneumogastrics had not, as far as I am aware, been ascertained. I therefore started at this point, and first performed that experiment. I found, however, that it was unattended with any appearance of sugar in the urine. In the operation that was performed, the division of the cord was effected between the third and fourth cervical vertebræ, so that artificial respiration had to be resorted to.

Although no diabetic effect was observed after division of the cord and pneumogastrics together, yet, after division of everything belonging to the nervous system in the neck, as by decapitation, the urine became in a very short time strongly saccharine, the circulation being kept up by the performance of artificial respiration.

The results obtained in these experiments tended to confirm the notion I had commenced with. Complete separation of the *medulla oblongata* from any possible connexion with the liver produced a saccharine state of the system, whilst division of the cord and pneumogastrics did not. Now, might not the sympathetic, which also runs through the neck, form the medium of communication sought for? This was the next question that naturally suggested itself, and had to be answered by experiment.

Division of the carotid sympathetic—that portion of the sympathetic system which passes down from the superior cervical ganglion in front of the vertebral column towards the chest—is an old experiment, and one that has often been performed by physiologists, on account of the interest it presents in connexion with the production of heat. It does not, however, occasion any perceptible effect on the urine.

But, there is yet another portion of the sympathetic system running through the neck; and this, previous to the prosecution of the present inquiry, had, so far as I know, enjoyed immunity from special attack by the experimentalist. This portion, consisting of the filaments accompanying the vertebral artery, might constitute the channel I was searching for. I therefore determined to effect a division of it, and, my first experiment resulted in rapidly occasioning a highly saccharine state of the urine.

Thus consisted, the origin of my experiments on the sympathetic, in connexion with the production of artificial diabetes. Such was also the process of reasoning that led me to the point. The evidence afforded by certain experiments had induced me to believe that the *medulla oblongata* in some way or other presided over the functional activity of the liver, so as to overcome the tendency of its amyloid substance to transform into sugar in obedience to the operation of the unopposed forces of

chemistry. I thought there must be a channel of nervous communication between centre and organ, which, if interrupted, would place the liver in the same condition as it is after death; and, on experimenting, I found, that dividing the vertebral portion of the sympathetic actually occasioned saccharine urine.

Upon witnessing this result, I was naturally inclined to hope, that everything had now been made clear. Although, however, it is a fact, which I have repeatedly verified, that lesion of the vertebral filaments of the sympathetic may produce saccharine urine; yet, subsequent research has brought to light other facts which do not harmonise with the notion that conducted me to the experiments, but show, that our knowledge upon the subject must still be regarded as incomplete.

The effect on the urine, produced by lesion of the vertebral portion of the sympathetic, is an exceedingly rapid one. Division of the filaments ascending from the superior thoracic ganglion towards the vertebral canal on each side of the neck, has given rise to urine, in half an hour's time, so highly charged with sugar as to occasion a copious yellow reduction with the copper solution. In an experiment, where the filaments on one side of the neck only, were at first divided, the urine at the end of an hour and a half, gave only a trace of saccharine reaction. The filaments on the other side of the neck being then divided, in half an hour's time, the urine had become intensely charged with sugar.

It may be mentioned, that division of these filaments belonging to the superior thoracic ganglion, always occasions a fatal pleurisy. The inflammation evidently results from the injury inflicted on the sympathetic, because, when the nerves have escaped being divided, as has sometimes happened, owing to the difficulty in seeing them, there has been no pleurisy set up.

On following the vertebral filaments upwards, and dividing them in different parts of their course through their canal, a circumstance was noticed, the explanation of which, I must own, I cannot at present conceive. In my first experiments, as I thought it was useless to attempt to tie the vertebral arteries after entering their canals, and as I could not isolate the sympathetic so as to effect its division, without dividing also the associated vessels; I considered it best, to ligature the two vertebrals before reaching their canals, and likewise the two carotids, on account of the freedom of anastomosis existing above.

It might have been imagined that on suddenly arresting these four streams of blood to the head, considerable disturbance of the cerebral functions would have been produced. Such, however, was not the case; indeed, scarcely any visible effect was perceptible. Now, it happened, where these vessels had been tied, that, on afterwards cutting or tearing through all the structures in the vertebral canal on each side of the neck, a strongly saccharine state of the urine was rapidly induced. For the production of this effect, however, the deligation of the arteries proved an indispensable part of the experiment; although, when this operation alone was performed, no diabetic result was observed.

To repeat, the conclusions furnished by the several experiments performed, stood thus: Careful deligation of the two vertebral (below their canal) and two carotid arteries did not occasion saccharine urine; nor, did simply tearing through everything traversing the vertebral canal on either side of the neck—an operation that I found might be performed without any material loss of blood being occasioned. When these two operations, however, were combined, sugar rapidly appeared in the urine. As I have said, I must confess, that I do not see the rationale of these results,

but it was some time before I could even make out anything at all definite towards reconciling the effects of the different experiments.

Lesion of other parts of the sympathetic besides its vertebral filaments, will occasion saccharine urine. Although division of the carotid sympathetic, in its course through the neck, does not produce any effect, yet removal or injury of the ganglion above—the superior cervical ganglion—may cause, in a very short time, the urine to be strongly charged with sugar. In one experiment, where I removed the ganglion of the right side only at first, the urine became in an hour's time intensely saccharine. As an effect of the operation also, the ear on the right side was rendered 3° (Fahr.) warmer than the other, the pupil more contracted, and the nostril drier. On the following day, the urine was still saccharine. The next day, however, the sugar had disappeared, and on the day succeeding this, the urine not being saccharine, the ganglion of the left side was removed. In half an hour's time, the urine had again become strongly charged with sugar.

The following is the amount of sugar found upon analysis in two experiments, where both ganglia were removed at once. In one, the urine, half an hour after the operation, contained 20·5 grains of sugar to the fluid ounce, and two hours after, 11·4 grs. In the other, 22·86 grs. per ounce one hour and twenty minutes after, and 34·08 grs. half an hour later.

There has been much diversity in the results of the experiments that I have conducted upon the sympathetic system in the chest below the superior thoracic ganglia. Division on one side has sometimes produced as rapid and strongly marked a diabetic effect, as has occurred after the other lesions of the sympathetic, to which I have referred. At other times, however, there have been only traces of

sugar discoverable in the urine; and at other times again, none at all. I have not found that the diabetic effect has been more frequently produced by division on both sides, than by division on one side only; nor, have I found that any difference has been made by which particular part of the sympathetic throughout its course, has been attacked.

In the case of the rabbit, diabetes is not so readily established by injuring the sympathetic as it is in the dog. The superior cervical ganglia are conspicuous bodies and easily reached. Their removal in the healthy and vigorous animal occasions saccharine urine, although the effect is not produced with anything like the rapidity that I have noticed in the dog. I have tried to divide the sympathetic at the lower part of the neck, but have met with difficulties in the performance of the operation that have prevented me from possessing a record of a fair experiment to speak of.

The diabetes resulting from all these operations on the sympathetic is quite of a temporary character. What the chain of phenomena is that determines the effect, constitutes the important problem that yet presents itself for solution. But, I have now strong reason to believe, that it does not depend, as I originally was inclined to think, on a simple interruption to the transmission of nervous force between the *medulla oblongata* and the liver.

I consider it an interesting and important fact, that the introduction of a sufficient quantity of carbonate of soda (200 grains) into the circulatory system, previous to injuring the sympathetic, checks the production of diabetes. This I have over and again verified in the case of the superior cervical ganglia. It is true, it must be borne in mind, that a diabetic effect does not universally follow removal of these ganglia, where no injection of carbonate of soda into the blood has been resorted to; but it generally does so: where, however, the injection has been pre-

viously employed, I can confidently look for an absence of any diabetic effect. In the experiments I have conducted, also, upon dividing the vertebral filaments of the superior thoracic ganglia, after the introduction of carbonate of soda into the system, the result has been identically the same.

Such are modes in which saccharine urine may be experimentally induced amongst the lower animals. It is thus evident, that there is a variety of ways in which the same effect may be occasioned, and, I think it probable, that in all, it may be subsequently proved to arise from a disturbance of the functional operations carried on in the liver. Sugar escaping from the organ, contrary to the natural order of occurrences, and reaching the circulation must, in obedience to physical laws, be eliminated with the urine. It is to saccharine urine as it occurs in the human subject constituting diabetes, that I shall now proceed to direct attention, and I propose to speak of it under the several heads of its seat, cause, symptoms or manifestations, and treatment.

PART III.

On the Pathology and Treatment of Diabetes.

Diabetes is the point, to the elucidation of which, the contents of the preceding pages have been directed. A long introductory preface, it is true, has been required to arrive at the point that has now been reached; but, as it is upon a correct physiology that a knowledge of the disease must be based, I have considered it necessary to enter as fully as I have done into the foregoing physiological details.

The disease consists of a defective performance of one of the functional operations of life; and, unless our *data* are correct concerning the nature of the occurrences that normally take place, it is not likely that we can form a just estimation of the manner in which the function is at fault. I believe the experimental evidence I have adduced will have the effect of carrying us back to the views entertained previous to the promulgation of the glyco-genic theory, which I cannot help thinking, therefore, has been leading us astray for some time past. My experiments decidedly tend in the main to confirm the opinions expressed by our countryman, Dr. Prout; from which, attention has been diverted by the doctrines of glyco-genesis. Our ideas in connection with the pathology of diabetes have been lately running upon sugar secretion in the liver, and sugar destruction in the lungs—processes not having in reality the existence that

has been latterly supposed; but founded on experiments that prove to be fallacious.

A preliminary investigation of the relations of sugar in the animal system has always appeared to me of the first importance in the study of diabetes; for, I have not seen how we could reasonably expect to make any real advance in the pathology of the complaint, until the physiology of the subject should be established upon a correct and substantial footing. It is true, the cause of diabetes might be, perchance, accidentally discovered; but, unless our knowledge of the phenomena naturally in occurrence be correct, we cannot, I repeat, expect to have any accurate idea of the manner in which the effect is produced. For this reason, I have felt that I could not justifiably curtail the space that has been devoted to the discussion regarding glyco-genesis. The facts I have revealed have thoroughly satisfied my own mind; and I cannot do otherwise than think that the present glyco-genic theory must be, sooner or later, entirely abandoned. The theory, in fact, is founded upon the condition of the liver and blood, discoverable by an ordinary examination after death. And, the result of such an examination, as I have shown, is not in accordance with the *ante-mortem* or physiological state. It must, therefore, be discarded from our consideration in forming a notion of what naturally occurs during life.

The theory founded upon the doctrine of glyco-genesis, referred diabetes; either, to a production of sugar in excess of its capacity for destruction; or, the production remaining the same, to an interference with its normal extent of disappearance. But, the production of sugar specially for subsequent destruction in the system must, if what I have brought forward prove substantial and true, be now left out of sight in the matter. There ought to be no sugar of any consideration in the blood, and, should there be any, it may

be considered as resulting from some unnatural circumstance leading to its production, and will immediately occasion, according to the extent of its presence, a more or less highly saccharine condition of the urine. Even the saccharine element derived from our food, ought to be assimilated and prevented from entering as such the general circulation. No matter from what source the sugar may be derived, the result of experimental evidence decidedly is, that whenever it reaches the general circulation, it is sure to be pumped off with the urine. There is no process by which, in any part of the circulatory system, sugar is known to undergo any distinctly appreciable extent of destruction. Sugar that has once escaped from the liver is to be detected alike in the blood throughout its whole circuit.

There is not a shadow of proof, as far as I am aware, to show that sugar is susceptible of direct oxidation in the economy; or, that as sugar it is capable of immediate administration towards the maintenance of animal heat. Liebig's theory about the combustion of sugar during respiration tallied admirably with the doctrine of glycogenesis; because, it provided a mode of usefully disposing of the material supposed to be so extensively produced by the liver and poured into the blood on its way to the lungs, where it was to be applied towards the service of heat production. It must be stated, however, that such a theory has no foundation in fact. Although the brilliancy and feasibility of Liebig's ideas, made so many converts to his physiological views, at the time they were first propounded, yet, upon the point in question, Dr. Prout stood forth as a stout opponent, as is evident from the following passage found as a note at p. 459, in the fifth edition of his work on Stomach and Renal Diseases: "The reader will observe that in the above as well as in "other parts of this volume, I have advanced an opinion

"regarding saccharine aliments totally at variance with "the prevailing doctrine of the day, viz., that the only use "of the saccharine class of aliments is to form animal heat "by combustion in the lungs. I regard this hypothesis in "its literal and general sense, as utterly at variance with "the experience and common sense of mankind; and have "no doubt that future physiologists will look back with "wonder that anything so absurd should have been ad- "vanced, much less adopted, in the present 'enlightened' "age."

Not only did Liebig's theory of sugar combustion usefully harmonise with the doctrines of glyco-genesis, but a portion of the experimental evidence brought forward in connection with glyco-genesis seemed to show that a destruction of sugar really took place in the lungs; and thus, that Liebig was supported in his view. It has been explained, however, how the evidence in support of sugar destruction in the lungs has rested upon a fallacious mode of experimenting. Acting upon the present knowledge we possess of the sources of error to avoid, it can no longer be found that there is any difference discoverable in the blood on either side of the lungs. Whatever sugar may be present in the blood of the right side of the heart will, as far as can be shown by analysis, be found to the same extent after this blood has traversed the lungs and reached the arterial system. Hence, if the liver possessed a sugar-forming function in accordance with the glyco-genic theory, the urine of us all would always present a strongly saccharine character. There not being the destruction of sugar going on in the lungs that was formerly supposed, it is apparent, that whatever sugar, no matter from what source, that reaches the general circulation, must be distributed through the system with the arterial blood. Sugar would thus be circulating through the kidney; and, on account

of its diffusibility, could not fail to escape and occasion a more or less saccharine state of the urine according to the existing saccharine quality of the blood.

Even the trace of sugar found naturally in the contents of the circulation, can be shown to be perceptibly drawn upon by the kidney; for, Dr. Bence Jones's experiments, in confirmation of those of Prof. Brücke, most satisfactorily prove, that a minute quantity of sugar, must now be ranked as one of the constituents of healthy urine.

The fact of sugar being susceptible of recognition as a minute constituent of healthy urine, I look upon as affording very strong evidence against the validity of the glycogenic theory. If the trace of sugar naturally existing in the blood cannot evade elimination from the system with the urine; what, it may be asked, might reasonably be expected to occur, if sugar were extensively manufactured for constant passage from the liver, as was formerly supposed? Sugar in quantity arriving in the general circulation would render us all inevitably Diabetics. For the existence of a glyco-genic function, such as we have latterly believed in, there must needs be also, a corresponding existence of a process, by which the sugar may be rapidly and effectually disposed of advantageously to the system after entering the circulation. For the occurrence, in fact, of sugar production in the liver, there must be a corresponding capacity for sugar destruction in the blood, before the kidneys are reached; or else, in compliance with physical laws, the sugar will escape with the urine and thus prove an unserviceable material as far as the economy of life is concerned.

Looking to the experimental results that have been mentioned in the physiological part of this work, the sequence of phenomena in connection with sugar, occurring in the

animal system under natural circumstances during life, would appear to be as follows.

Sugar ingested as such, or derived from the metamorphosis of starch in the alimentary tract, obtains entrance into the system, by the physical principle of diffusion, through the portal vessels. It becomes thus carried to the liver, by the cells of which, it is extracted from the blood and transformed into amyloid substance. Instead of the liver allowing sugar to pass through it, and also producing sugar itself, it transforms that which reaches it into amyloid substance.

The close and extensive connection between the hepatic cells and the capillary blood-vessels in the lobules of the liver, is admirably calculated for enabling a material to be thoroughly picked out from the blood. That sugar is thus detained and converted into amyloid substance, I hold to be proved by the effect of a saccharine alimentation that has been already described. But, the power of the liver in this respect is not unlimited ; for, although the introduction of sugar into the system in moderate quantity through the natural channel, does not lead to a saccharine impregnation of the general circulation, yet, when ingested beyond a certain amount, so that a very large quantity would be conveyed to the liver, a portion may pass through and give to the blood, and thence to the urine, a more or less highly saccharine quality. The injection of sugar directly into a branch of the portal vein will occasion this occurrence ; and, in the case of the animals referred to at a former page—the dogs that had been kept for a few days on animal food with a large admixture of cane-sugar, the urine in three out of the four instances was distinctly charged with grape-sugar. In some of the rabbits, also, that I have purposely fed on starch and sugar only, for a short time, the urine has presented indications of being impregnated with sugar.

Amyloid substance, however, is also found in the liver, notwithstanding that no saccharine element may have entered into the constitution of the food. It is evident, therefore, that there must be some other source for its production; and, I am induced to believe, that it is derived likewise, from some of the products arising out of the changes that animal food undergoes, as also, from some of the products resulting from the wear and tear or the retrograde metamorphosis of the tissues. I have no direct experimental proof, it is true, to adduce on this point, but it has certainly seemed to me, that one purpose of the liver is to extract from the blood some of the materials derived from the more highly organized compounds in their downward course in the system, and to convert them into amyloid substance, thereby placing them in a position for still further use during life: thus, in fact, economising material, by rendering susceptible of appropriation to another purpose, that which might otherwise be simply discharged from the body.

Thus far, the sequence of phenomena in connection with the physiological appropriation of sugar may be very satisfactorily traced. That the amyloid substance is not specially designed to come back into sugar, I maintain for the reasons that have been so fully discussed. I have no doubt, that its destination is for service towards heat production; but, what the series of changes that occur, until this final purpose is accomplished, remains as yet to be made out. The two ends and the front links of the chain, I think, we may fairly consider we hold in our possession, but there are yet a few links wanting to give us the whole.

Out of sugar, it is now admitted, that the animal can form fat. Bees produce wax when limited for food to sugar-candy. The herbivora get fat on food that abounds with sugar or starch, and practically speaking contains no fat. The West

Indian negroes, we are told, grow enormously bloated and fat at the season when the juice of the sugar-cane is expressed, from their habit of licking the ends of the stick.

Sugar, then, leads to fat, and it has been shown as a primary step after absorption from the alimentary tract to be metamorphosed into amyloid substance. The missing links of the chain consist of those between amyloid substance and fat. Now, not only may the copious ingestion of sugar or starch lead to an enormous accumulation of amyloid substance in the liver, but also of fat, as is strikingly exemplified by the occurrence that is observed to result from the process to which geese are submitted for obtaining the *foie gras*. It would thus seem, that sugar may go on in the liver to fat, but whether it all proceeds in the liver so far as this, is, I think, open to very great doubt. I must needs, however, here desist from continuing this point further, for it would be unwise to carry our discussion into a subject, upon which, it must be admitted, the evidence of fact is required to remove it from the sphere of speculation. It will have sufficed for my purpose in connection with Diabetes, if I have shown, that sugar must have a destination otherwise than that provided for it by the glyco-genic theory, for, on this point will turn our notions, regarding the pathology of the complaint.

Opinions have varied at different epochs as to the organ at fault in Diabetes. At one period the kidneys were looked upon as constituting the seat of the disease. Then, it was discovered that the sugar existed in the blood, and that the renal organs only performed a separating or excretory office. The announcement by M'Gregor ('London Medical Gazette,' May, 1837) that sugar was to be found in the Diabetic, although not in the healthy stomach, after the ingestion of animal food, gave rise to the idea that the seat of formation of the sugar was in the digestive organs.

M'Gregor, after giving an emetic and purgative to a healthy man, fed him upon roast beef and water exclusively for three days. At the end of this period, and three hours after a meal, a second emetic was given. The vomited matters gave no evidence of the presence of sugar. A Diabetic was submitted to a similar treatment, and the vomited matter, ejected after the second emetic, was found to yield an indication of being impregnated with sugar. In the conclusion drawn from these experiments, it was omitted to take into consideration the fact, that in the Diabetic, the blood being saccharine, the secretions derived from it, are also, nearly all, if not all of them likewise saccharine. This has been shown by Bernard to be certainly the case with the gastric juice; and thus is satisfactorily accounted for, the presence of sugar in the stomach after a meal of purely animal food; without supposing this sugar to be formed as the result of a perverted digestion—a notion which receives no support from the evidence afforded by the researches of the modern day.

Prout (‘Stomach and Renal Diseases,’ 5th edit., p. 38), when speaking of the treatment of Diabetes, says: “The “facts and observations I have to offer on the subject “are founded on the opinion already advanced, viz., that “Diabetes is nothing more nor less than a form of dys-“pepsia; that this dyspepsia principally consists in a diffi-“culty of assimilating the saccharine alimentary principle; “and that like all other forms of dyspepsia, whether it be “an inherited or an induced affection, Diabetes is liable “to be much modified and aggravated by concomitant “circumstances.” Other passages from the same work might be quoted to show, that Dr. Prout regarded Diabetes as resulting from a defective action of the stomach; and yet, in a note at p. 37, he says, “Functionally speaking, “I am induced by long attention to believe that the liver

"is always deeply involved in Diabetes." It appears, however, from the text, that this remark is intended to imply, that the liver is involved by the constant passage of sugar through the sanguiferous system, and not, that such functional involvement has anything to do primarily with the complaint.

Looking at all the facts that have been brought to light by modern research, I am strongly inclined to believe that the organ whose action is essentially at fault in Diabetes is the liver. I know of nothing to show that the stomach exercises any special assimilating or converting influence over the saccharine principle. In compliance with physical laws, sugar presenting such highly diffusible properties as it does, cannot, when existing in the stomach or intestine, escape entering the circulation and being thus carried to the liver. Now, the liver is the organ that appears to exert an assimilating power over the saccharine element of our food. Whilst traversing the capillaries of the organ, it seems to be picked out by the hepatic cells and submitted to transformation. This immediate transformation is a necessary concomitant, because, sugar could not remain as such in the hepatic cells. From its diffusibility, it would be carried back again into the circulating current. But, the evidence afforded by experiment goes to show, that it does not reach the general circulation, and nothing is more clearly established to my own mind, than, that one source of the amyloid substance of the liver is saccharine matter. The numerous results I have referred to, in a previous part of this volume, all harmonise with each other and prove that the ingestion of starch and sugar leads to a large accumulation of amyloid substance in the liver. In accordance with the properties required of it, for retention in the hepatic cells, this amyloid substance occupies a position the very antithesis of sugar as regards diffusibility. Like albumen and unlike sugar it is devoid of a disposition to diffuse through animal membranes.

Although there appear to be good grounds for believing, that it is upon a defective action of the liver that the phenomena of Diabetes depend, yet, when we come to speak of the circumstances determining this defective action we arrive at a point that, it must be admitted, still remains without a clue to its solution. If, however, experimental research has not yet disclosed the nature of the pathology of Diabetes, I believe it has paved the way to a position, from which the disease is more likely to be investigated with success. It is fair to infer, that as long as our notions upon the physiology of this subject were based on erroneous conclusions, no real advance was likely to be made in its pathology unless by some improbable fortuitous discovery.

As to the cause of Diabetes, I take it that in different cases different causes may be concerned in the production of the disease; just as we learn from experiment, that there are various ways, by which saccharine urine may be artificially occasioned.

Dr. Prout thought, that in many instances he had been distinctly able to trace Diabetes to exposure to cold; or, sometimes to rheumatic attacks brought on by exposure to cold and moisture. For the most part, however, it must be admitted, that no reasonable cause can be assigned for the commencement of the complaint; and, supposing it to have been traced to exposure to cold, no clue has yet been obtained of the manner in which the effect is produced.

Bernard in his 'Leçons de Physiologie Experimentale,' Paris, 1855, p. 346, has mentioned a case, where a saccharine state of the urine followed a blow over the region of the liver. He says "On me citait dernièrement le cas d'un "individu qui avait reçu un coup de pied de cheval dans "l'hypochondre droit, et qui, à la suite de cette blessure, "avait présenté du sucre dans ses urines; mais ce symp- "tôme avait disparu lorsque le malade avait été guéri "de sa contusion, seulement il était resté polyurique."

This must be looked upon as an interesting case, because, it would seem to form an instance of Diabetic effect, produced through the operation of a direct influence upon the organ whose action has been supposed to be at fault in the complaint.

Cases may be cited, which point strongly to Diabetes being sometimes connected, in some way or other, with cerebral disease.* I believe, as attention is directed to this point, evidence will accumulate and strengthen this statement. An instance has fallen under my personal notice, where the disease in a severe form immediately followed a violent blow upon the head. The following cases have also been mentioned to me in which Diabetes was associated with cerebral disease. A late alderman of the City of London was attacked with cerebral hemiphlegia. His urine was tested by Dr. Barlow (from whom I obtained these particulars) at the period of attack, and found to be

* Whilst these sheets were passing through the press, a case was received, under Mr. Poland's care, into Guy's Hospital, where saccharine urine immediately followed a fatal injury to the head. A female child, four years of age, had been run over, and sustained a fracture of the skull. She remained quite insensible and could not swallow. Her respiration was of a sobbing character but not stertorous, and a little blood ran from the nose and mouth. There were no convulsions. The child died four hours and a half after admission. On examining the head, it was found by Dr. Wilks that the skull was fractured through the base. There was a little blood external to the *dura mater*, but the *dura mater* itself was uninjured. The surface of the base of the brain was slightly bruised. The ventricles contained a little bloody serum. The fornix was slightly ecchymosed. The floor of the fourth ventricle was ecchymosed from slight effusion of blood into its substance. The urine contained in the bladder, I found to be charged with sugar to the extent of five grains and a half to the ounce. A difference of opinion may arise as to the precise cause that determined this presence of sugar in the urine. For myself, I am inclined to think that it probably resulted from a secondary influence of the injury upon the liver through the circulation. Sudden congestion of the liver through an impediment to the circulation has been found experimentally to produce saccharine urine; and, in such a case as the one just referred to, I do not doubt that the breathing was so influenced as to create a serious disturbance to the free passage of blood through the lungs. I am strengthened in this opinion by the fact that the urine was also in a very slight degree albuminous.

free from sugar. There had also been nothing, from the symptoms and history, to lead to the suspicion that sugar would be found. Shortly afterwards, however, strongly marked Diabetes set in. A member of the medical profession, who was seen by Dr. Gull, was seized at the age of 52 with an apoplectic fit, from which he recovered, with hemiphlegia, however, of the left side of the body remaining behind. Five weeks after the fit, this person, who had never previously presented any symptom of Diabetes, began rapidly to emaciate, which led to an examination of the urine being made. A highly saccharine state of it was found to exist.

According to Dr. Prout, there is a form of Diabetes, which occurs in connection with gout and dyspepsia. He says* "Indeed a saccharine condition of the urine exists "in dyspeptic and gouty individuals much oftener than is "supposed; and hundreds pass many years of their lives "with this symptom more or less constantly present, who "are quite unaware of it, till the quantity of urine becomes "increased."

A saccharine state of the urine has long since been spoken of, as a frequent if not universal accompaniment of malignant boils and carbuncles. A case in point has been recently referred to in the 'Medical Times and Gazette,' December 21st, 1861. The patient was 73 years of age and the subject of hemiplegia. He had anthrax in the lumbar region, during which a considerable quantity of sugar was found in the urine. On recovering from the anthrax, the sugar is stated to have disappeared. The hemiplegia still persisted. The case is given as an extract from the 'Gazette Hebdomadaire.'

The symptoms and disturbances in Diabetes are mainly, if not entirely, dependent on the extensive presence in the blood, of a material which ought, in the natural course of

* Prout, 'On Stomach and Renal Diseases,' 5th ed., p. 32.

events, to have been directed towards another end. The presence of sugar in the blood, determines a saccharine quality of the urine, and, it is this, which is regarded as constituting the cardinal feature of the complaint. Other circumstances being equal, the amount of sugar eliminated in the urine, may be looked upon as affording an index of the extent of functional defect existing, which varies considerably in different cases and at different periods in the same case. North (Case I, appendix) passed nine and ten thousand grains of sugar a day upon a full mixed diet, at the commencement of his treatment under my care; and, from eight to nine thousand grains, some weeks later, when the same diet was resumed; his condition having meanwhile improved, by considerable restriction from vegetable food. In Case II, a man of less weight than North, the amount upon similar food was never below eleven thousand, and upon one occasion, reached nineteen thousand grains of sugar for the day. In Case III, an elderly man, the amount of sugar was only three and four thousand grains for the same period and under the same diet. In Case IV, the patient was kept for several months upon an animal regimen. On casting the eye through the details of the report, a considerable variation within certain limits is noticeable in the quantity of sugar voided at different periods, notwithstanding there was the utmost regularity and scrupulousness in the diet. At one time, there was a gradual descent in the amount of sugar, until it reached only 116 grains for the twenty-four hours. The corresponding quantity of urine was only thirty-three ounces, and the proportion of sugar to the ounce 3·52 grains. During the few following days, the sugar again rose in quantity, and reached as high as 479 grains for the day. It then fell to 115 grains, the quantity of urine being thirty-nine ounces, and the proportion of sugar to the ounce 2·96 grains. I had been cherishing the idea, on noticing

these descents to so insignificant an amount for the twenty-four hours, that the sugar was about to disappear altogether; but, an attack of diarrhœa supervening, it underwent a rapid elevation, and continued above what it had been, shortly before the case had presented so favorable a turn.

In some cases of Diabetes, the morbid condition of the urine temporarily disappears *in toto*, under abstinence from food containing starch and sugar. Of the four cases just referred to, one was of this description. In Cases I, II, and IV, restriction to an animal diet never produced a total disappearance of the sugar; but in Case III, under the animal regimen, there was not a trace of sugar to be discovered in the urine. The sugar, however, returned in marked quantity, upon two ounces of ordinary bread a day, for a couple of days, being allowed, again disappearing, on the withdrawal of the bread being made. With two ounces of bran biscuits a day instead of the bread, the urine remained devoid of sugar.

Two cases have also recently fallen under my notice, where the morbid condition of the urine was wholly dependent on the food.

In one of them, the history the person gave me, was, that he had been Diabetic about a couple of years, and had latterly been scrupulously confining himself to an animal regimen with the bran biscuit. His urine was normal in quantity, of ordinary colour, and threw down a deposit of lithates. He presented the opposite appearance to that of a Diabetic, being exceedingly stout, and complained of none of the ordinary symptoms of Diabetes. I, therefore, recommended him to try the effect of partaking moderately of wheaten bread with his meals for one day. The urine he passed, was collected during the time, and found to give a strong reaction with the copper solution.

In the other case, the person's appearance also, was

the reverse of that of an ordinary Diabetic. He had only been ill a few weeks, and had been told he was suffering from Diabetes, for which he had been keeping himself to animal food and bran biscuit. He had never passed an inordinate quantity of urine, nor had he experienced any particular hunger or thirst. The urine he was voiding was perfectly free from sugar. On recommending him to resume an ordinary diet, the urine he brought me in a few days' time contained 2·85 grains of sugar to the ounce. Passing, as he informed me, about two quarts during the twenty-four hours, would give about 228 grains of sugar for this period. He now returned to the animal food and bran biscuit, and the urine three days afterwards, contained only traces of sugar. The urine subsequently losing this trace, and continuing free from sugar for a fortnight, he was allowed to partake sparingly of ordinary vegetable food. For a week, his diet consisted of about half a pound of potatoes and half a pound of bread a day, with meat, fish, greens, coffee and tea. At the end of the week, his urine was devoid of sugar. During the following week, the allowance of potatoes and bread was doubled, and the urine remained quite free from sugar. The week afterwards, beer was taken in addition, and still without leading to the elimination of sugar with the urine. Considering himself quite well, he now ceased to attend.

There is, thus, considerable variation in the extent of defective or perverted action that exists in Diabetes. The disease cannot be spoken of, as simply an absence of power to assimilate the saccharine principle. It would seem rather to consist of a defect or aberration, which under different circumstances, presents a great variation in its degree of manifestation. Upon a given regimen, very different quantities of sugar may be, in different cases, and at different periods in the same case, eliminated.

In some cases, there is a total want of assimilative power over the saccharine element, so that, whatever belonging to this class of food is consumed, passes through the system unappropriated; and, something more than this, for sugar is voided which does not come from direct ingestion with the food. In other cases, the want of assimilative power over the saccharine element of the food, constitutes the whole manifestation of the disease, and, no sugar is voided when abstinence from saccharine and starchy materials is observed. In other cases, again, the deviation from health may be still less marked. A certain amount of vegetable food may be taken without giving to the urine a saccharine character, but, if the patient go beyond this extent, sugar escapes from the body unappropriated. With all of us, it would seem, the extent of assimilative power over the saccharine element is not unlimited, for sugar has been noticed to pass with the urine, as the result of very copious introduction into the alimentary tract, especially when fasting has been observed for a short time. In the latter class of Diabetics, however, this assimilative power over sugar exhibits a degree of limitation beyond that belonging to the ordinary state of health.

The continuance of sugar in the urine, notwithstanding none has been ingested with the food, must I think be considered likewise to depend upon a defect of assimilation. In animals that have been kept upon a purely animal diet, the liver is found to contain amyloid substance, which, under such circumstances, would appear to be derived from the products of disintegration of the tissues and of the metamorphosis of the food. Now, supposing, from a perverted exercise of the functional operations carried on in the liver, this amyloid substance should descend into sugar, the elimination of sugar with the urine will follow as a necessary consequence.

The severity of the symptoms in Diabetes is, as a rule, proportionate to the quantity of sugar reaching the circulation and passing through the system. In the cases that have fallen under my observation, the disease has not been attended with the elimination of so much sugar, when occurring in elderly, as in young and middle-aged persons. The disease, in fact, has seemed to me, to manifest a disposition, to assume a less intense form in elderly than in earlier life. In proportion as there has been less sugar produced, amongst those of advanced years; so, there has not been that height of inconvenience complained of, as is so frequently the case when the disease occurs during youth and middle age. Unless the disease is held in check by appropriate treatment, its tendency (speaking from the cases I have seen) would seem as a rule to be, to assume an intense form, to run a rapid course, and to occasion the most urgent symptoms, when it attacks young and middle-aged persons; whilst, it is not uncommon amongst elderly people for the disease, from the beginning, never to have given rise to any serious inconvenience, and not to be attended with any marked wasting of the body, such as is so constantly the case in earlier years. Judging from what I have seen, then, I am inclined to believe, that the complaint has a tendency to present a less acute form, where the age of the person is advanced; and, I may add, at the same time to prove more amenable to treatment.

The sugar which reaches the general circulation, is distributed throughout the whole system in Diabetes. I have several times examined the blood of Diabetics that has been removed by cupping and by venesection, and have found it charged with sugar so as to give a copious orange-yellow reduction with the cupro-potassic solution. In one case, where I made a quantitative analysis, the blood, which had been abstracted from the loins by cupping, contained

53 hundredths, or just over half a grain of sugar per cent. This would give about two and a half grains of sugar to the fluid ounce. The sugar has long been known, to be susceptible of being obtained from the blood in a crystalline state, and I have so procured it myself.

Sugar being present in the blood, escapes through other channels besides the renal organs. M'Gregor detected it in the alvine evacuations. Bernard has shown that it is present in the gastric juice, but says that it is not to be met with in the saliva, the pancreatic juice, the tears, the sweat,* or the bile. He says, also, that the cerebro-spinal fluid and serous effusions are charged with sugar, when this principle is distributed through the system as it is in Diabetes. Lehman affirms that he has found sugar in the saliva during Diabetes, but Bernard suggests that it is necessary not to confound mucus from the lungs, which contains sugar, with true saliva. I have examined the saliva in two cases of Diabetes. I found in one, no evidence of the presence of sugar; whilst in the other, my test afforded an indication of a trace.

I once had an opportunity of examining pus that had formed during the existence of Diabetes. The case was that of a girl about twelve or thirteen years of age, and the purulent matter was obtained from an abscess situated on the head. The test afforded an indication of sugar.

The blood of Diabetics has been spoken of (Todd's 'Cyclopædia of Anatomy and Physiology,' art. Blood, by Dr. B. Guy Babington) as presenting a peculiarity in respect of a milky character of its serum. My attention has been

* Dr. Parkes, in his work on the Urine, 1861, p. 353, says—"That sugar is found occasionally in the sweat is generally admitted. Fletcher ('Med. Times,' 1847, p. 394) obtained in forty-eight hours six and a half grains of sugar from a piece of flannel, three inches square, placed in the axilla. On one of my patients, a boy without phthisis, who was made to perspire profusely by the hot-air bath, Ranke found no sugar."

given to this point, in the opportunities that have occurred to me, of examining Diabetic blood removed by bleeding and cupping. In two instances, I have observed this milky character very strongly marked; whilst in others, the serum has only been slightly lactescent, or not lactescent at all.

I am hardly inclined to think, that there can be anything special about the milkiness of the serum in Diabetes, because, I have frequently noticed in my physiological experiments, and the same has been noticed by others upon the human subject; that, when blood has been removed a few hours after the ingestion of food, especially of food rich in fatty matter, the serum has been strongly lactescent, and on being allowed to repose, has given rise to an accumulation of a thickish cream-like layer upon the surface. This cream-like layer, on being examined microscopically, has presented the appearance of the molecular base of chyle, and doubtless, therefore, has been directly derived from the chyle. During fasting, there is no such appearance to be observed. Now, Diabetics usually consume a large quantity of food, and particularly of food likely to lead to an extensive introduction of fat into the system. The milkiness of their blood, therefore, may be quite unconnected with any peculiarity belonging to their disease, but simply dependent on the flow of fatty matter into the blood through the chyle, in accordance with what can be observed under natural circumstances to take place.

Where the abstraction of blood is resorted to, in acute febrile and inflammatory complaints, the circumstances of the case would scarcely, if ever, allow us to look for a milkiness of the serum as likely to be found. Such, being the complaints in which the removal of blood is by far the most general, we are afforded an explanation why the character of milkiness is not of more common observation than it is. I do not know whether it has any bearing in con-

necting the liver with the complaint; but, it may be mentioned that I have noticed a height of yellow tinge in the serum of Diabetic blood that I have not observed under ordinary circumstances.

I look upon the presence of sugar in the blood as the cause of those structural and functional disturbances that are so common in Diabetes. In Diabetes, as in albuminuria, there is an unusual susceptibility to the supervention of local inflammatory action. A cause or a circumstance that would produce no injurious effect upon a healthy system, is sufficient to set up the most serious consequences during the existence of the highly saccharine state of the blood that belongs to the severe form of Diabetes allowed to remain in an uncontrolled state. Dr. Prout very appropriately speaks of the frail tenure of life held by Diabetics, and gives cases to show how they exist, as it were, on the brink of a precipice, the fatigue and excitement of a long journey to consult him, having proved sufficient to occasion a rapidly fatal result in four instances during the space of a few years.

But at the same time, it must be admitted, that in some cases there exists, a source of interference with nutrition, beyond what can be accounted for, by the mere presence of sugar in the blood. For, although, the sugar may be kept down below what is produced by persons, where no material disturbance is perceptible, yet, the disease advances in weakening and prostrating the patient. It would seem in these cases, as if the cause which determines the saccharine state of the blood also does more than this, and affects deeply the processes of nutrition. Should such, in reality, be the case, the saccharine state of the blood would then form only one phase or manifestation of the complaint.

The most frequent mode of unfavorable termination of Diabetes is by disease of the pulmonary organs. This dis-

ease of the lungs is commonly spoken of as phthisis; but, although it runs the same course, and presents the same symptoms as phthisis, yet, it seems in reality to consist of a simple chronic inflammation; leading to the breaking down of the lung tissue, and the formation of cavities; and produced by the state of the blood, and not by any true or strumous tubercular deposit. I have witnessed the examination of several cases of Diabetes where death has resulted from lung disease, and there has been no sign of the existence of tubercle. Dr. Wilks' experience is also to the effect that it is not the true tubercular deposit that is met with in the lungs of Diabetics who have died from pulmonary disease. There is a deposit around the cavities, which is often spoken of as tubercle, but which consists in reality of a simple inflammatory production.

I recognise, then, the so-called phthisis of Diabetes as a chronic inflammatory affection, dependent on the presence of such an amount of sugar in the blood, as to alter its natural quality and render it unfit for the healthy discharge of its functions. We must all admit that a certain constitution of the blood is required to enable it to administer to the healthy performance of nutrition, and the several functions of life. Now, in physiology it is agreed upon, that the fulfilment of the processes resulting from a natural relationship between blood and tissues, creates a force which materially aids the circulation in the vessels. By a disturbance of this relationship, it is easy to understand how this force may be weakened: and thus, a cause that would not affect a healthy individual may operate in Diabetes, and produce a local congestion, which is followed by inflammatory action. In this way, I believe, is to be satisfactorily accounted for, the frequent association of inflammatory mischief of the lungs with the Diabetic complaint.

My colleague, Dr. Gull, has mentioned to me an inte-

resting case bearing on the point to which I have just been referring. A gentleman was suddenly seized with swelling of the right leg, and the circulation through the limb seemed to be arrested. The urine was found extensively saccharine. Recovery from the swelling of the leg took place, but confirmed Diabetes was established. A year afterwards, from exposure to cold, an attack of sudden hypostatic engorgement of the lungs took place, which, within forty-eight hours, carried the patient off without any sign of pyrexia.

This case tends strikingly to show, upon what a delicate balance the circulation in Diabetics hinges. From the altered constitution of the blood, its aptitude for administering to healthy nutrition is so weakened, that a comparatively slight disturbance is sufficient to lead to an arrest of, or such an interference with the process, as to occasion congestion or even stagnation of the circulatory current.

Dr. Gull has also mentioned to me an instance where he warned the friends of a patient—a medical man—against resorting to the application of a blister for cerebral disease; on account of the presence of sugar in the urine. A blister was nevertheless subsequently applied to the back of the neck. A large carbuncle followed, which caused the death of the patient. I am informed also, from the same source, that upon some few occasions besides this, gangrenous inflammation has been personally witnessed in association with the Diabetic state.

Although the lungs form the favorite seat of secondary implication in Diabetes, yet instances are frequently occurring of congestions and inflammations taking place in other parts. Diarrhœa is not an uncommon concomitant of Diabetes, and I can recall to mind some few cases amongst those I have seen, where a fatal termination has occurred from the prostration occasioned by such disorder. In the

post-mortem examinations, there has been no ulceration discoverable, only a preternatural vascularity of the intestinal tract.

Œdema of the lower extremities has been sometimes observed to supervene upon Diabetes, and granular disease of the kidneys is mentioned amongst the appearances that have occasionally been seen after death.

The anatomical appearances up to the present recognised, do not afford any assistance towards unravelling the nature of Diabetes. Nothing has yet been discovered to connect the disease with any primary structural alteration. A functional derangement exists, which cannot, as far as our knowledge yet extends, be spoken of as associated with any particular anatomical change. All that has been recognised in *post-mortem* examinations, consists of effects dependent upon the disturbed functional operation constituting the disease. In all the inspections that I have witnessed at Guy's Hospital, the patients have died either from lung mischief, diarrhœa, or a gradual atrophy attended with a corresponding prostration of the vital powers.

A little further on, will be found the particulars concerning the state of the solids and fluids, in what few cases the *post-mortem* room of Guy's Hospital has afforded me an opportunity of examining. It will be observed, that sugar may be either absent, or present, in the body after death. This depends, I imagine, upon the mode in which the patient dies. It has long since been known, that upon a severe form of an acute disease occurring to a Diabetic, the sugar may entirely disappear from the urine, to reappear, however, on recovery from the incidental complaint. M. Rayer has particularly drawn attention to this point, and I once saw in his service at La Charité, in Paris, a middle-aged female labouring under Diabetes, attacked

whilst in the hospital with smallpox. The urine, which had been previously highly charged with sugar, became quite free from it. The patient very soon died, so that no opportunity was afforded to watch its reappearance.

In a case, however, recently under Dr. Barlow in Guy's Hospital, the patient was attacked with typhoid fever during the prevalence of an outbreak in the ward. He was confined to his bed for several days, but ultimately recovered. I tested his urine on a few occasions, and found always a copious indication of sugar.

Sugar has not unfrequently been known to disappear from the urine a few hours, or even a few days before death in Diabetes, and I apprehend the rationale in such cases will be the same as with the occurrence of the similar phenomenon, under the supervention of an acute disease. It may be mentioned as a significant fact in connection with this point, that under morbid conditions of the system, the amyloid substance is found to disappear entirely from the liver.

Dr. Wilks is of opinion that the Diabetic liver presents a difference to the eye that enables it to be recognised from others. It is firm, tough, and homogeneous or uniform in appearance, and dark in colour. The bile, in nearly all the cases I have specially looked to it, has presented a striking appearance, resembling more a rhubarb mixture than anything else I could compare it to. On standing, it has allowed a pretty copious sediment to fall, which, examined microscopically, has been found to consist of columnar-shaped epithelial particles, with yellow amorphous or granular-looking matter.

Examination of solids and fluids of cases from the post-mortem room of Guy's Hospital.

May 27th, 1857.—Wm. T—, æt. 28. Diabetes with pulmonary complication. Urine passed on the day prior to death

gave a strong reaction of sugar. The lungs were found in an advanced state of disorganization from pneumonic phthisis.

Urine, of which there was but little, contained in the bladder after death, saccharine and of a strong colour and smell.

Liver, weight 3 lbs. 1 oz., of a darkish colour and homogeneous appearance. No reaction of sugar. Microscopic appearance—liver cells faint, nuclei distinct; a large number of regular-sized nuclei-looking bodies distributed over the field; small free fat particles.

Bile, thick and opaque, and of a deep-red or orange-red colour. Solid particles of the same colour were held in suspension, giving it the appearance of a rhubarb mixture. No sugar. Examined microscopically, there was yellow amorphous matter to be seen, with a few well-marked crystals of the triple phosphate.

Cardiac blood, no reaction of sugar.

Kidneys, weight $11\frac{1}{2}$ oz., no reaction of sugar.

Spleen, lungs, pancreas, submaxillary gland, muscular tissue of heart, brain, and cerebro-spinal fluid, examined and found to be free from sugar.

July 14th, 1857.—H. M—, æt. 13. Had been in a very weak condition for some time, but died at last very suddenly. Lungs quite healthy with the exception of a few lobules in the left lung, which were in an early stage of red hepatization. No tubercles discoverable in any part.

Liver strongly charged with sugar, and of a tough or somewhat fleshy or leathery consistence. Microscopic appearance—liver-cells very distinct; fat-granules in the interior of the cells, and fat-globules of different sizes distributed over the field between the cells.

Bile of a very peculiar appearance, a quantity of brownish-yellow particles being suspended in a slightly viscid liquid.

Kidneys gave a moderate reaction of sugar.

January 18th, 1859.—David E—, æt. 37. Diabetes of several months' standing. Three days' prior to death seized with profuse diarrhœa, which so enfeebled him that he gradually sank.

Lungs found healthy, with the exception of a few small patches of recent pneumonia.

Urine from the bladder after death strongly saccharine.

Liver, weight 3 lbs. 8 oz., strong saccharine reaction. Examined microscopically, the liver-cells were found irregular, smallish, and rather indistinct. No nuclei were to be seen in them, but granular matter and small fat-globules. A great number of various sized fat-globules were distributed throughout the field in a free state between the cells.

Bile of the consistence of, but of a rather paler colour than treacle.

Cardiac blood strongly saccharine.

November 13th, 1859.—H. F—, æt. 31. Death from complication with pulmonary disease and diarrhœa. Four days before death, which was the last time the urine was tested during life, there was a copious presence of sugar.

Urine removed from the bladder after death was devoid of sugar.

Liver free from sugar.

Cardiac blood free from sugar.

December 14th, 1859.—Sarah F—, æt. 17. Diabetes complicated with epilepsy. Two days prior to death took cold, had a fit and became seriously ill. Symptoms of pulmonary mischief set in, which increased till death took place on the 15th. *Post-mortem ;* signs of recent pleurisy on the left side, with a portion of the lung in a state of red hepatization; bronchial tubes inflamed.

Urine from the bladder after death strongly saccharine.

Liver strongly saccharine. No amyloid substance.

Bile of a reddish-brown colour, allowing a reddish deposit to subside. Obtained no reaction of sugar with the copper test. Pettenkofer's test reacted for cholic acid.

Kidneys, strong reaction of sugar.

Cardiac blood strongly saccharine.

Brain tissue strongly saccharine.

Muscular tissue of heart strongly saccharine.

Spleen strongly saccharine.

December 6th, 1860.—T. D—, æt. 51. Death from an old-standing lung complication. Body extremely wasted.

Kidneys, no trace of sugar discoverable.

Liver, no trace of sugar. The liver after being perfectly dried yielded 5·2 per cent. of fat (material dissolved by ether).

October 19th, 1861.—George W—, æt. 24. Three days before death, was taken with symptoms of pneumonia and profuse diarrhœa.

Lungs found hepatized in portions. No tubercle, or evidence of old disease.

Urine from the bladder after death, contained sugar in the proportion of 19 grains to the ounce.

Liver contained 0·62 per cent. of sugar, and after being perfectly dried yielded 10·5 per cent. of fat (material taken up by ether). Weight 3 lbs. $4\frac{1}{4}$ oz. Microscopic appearance —cells rather transparent, containing minute fat particles which seemed rather sparingly present; free nuclei-looking bodies.

Bile rather viscid, and allowing a brownish-red sediment to subside. This sediment, examined microscopically, displayed yellow granular matter, columnar epithelial cells, and colourless roundish granular-looking cells of various sizes. Reaction of cholic acid with Pettenkofer's test.

Kidneys, weight together $10\frac{3}{4}$ oz., strongly saccharine.

Cardiac blood strongly saccharine.

February 17th, 1862.—Wm. P—, æt. 38. Diabetes, with no symptoms of complication. Death quite sudden. Body not emaciated.

Lungs quite healthy.

Liver, strong reaction of sugar. No amyloid substance. Examined microscopically, the liver-cells were irregular in shape and charged sparingly with fat particles which were small in size. There were but few free fat particles distributed over the field. Besides the cells, blood corpuscules, and fat particles, there were many free nuclei-looking bodies present.

Bile of a deep green colour with brownish tinge, and of a rather viscid consistence. Examined microscopically, there were only some very small, yellow, irregular-shaped particles to be seen.

Kidney, strong reaction of sugar.

Spleen, no reaction of sugar.

Amongst the signs or symptoms of Diabetes the state of the urine constitutes the most characteristic feature of the

complaint. It does not, however, form the disease; being only one expression of a disorder which exists elsewhere. The urine is increased in quantity and altered in quality; the latter circumstance giving rise to, and in general terms, it may be said, regulating the former. The quantity of urine which a Diabetic sometimes passes is truly enormous. I have known, myself, as much as twenty pints to be voided in the twenty-four hours, but cases are recorded where the quantity has been even considerably higher than this. By dietetic measures in Case IV, Appendix, the amount of urine was reduced, at one time, to as low as 33 oz. It is the increase in the quantity of urine, with the thirst that is incidental to it, that usually first arouses the patient's attention, and leads him to believe that all is not right.

The quantity of urine varies pretty closely with the amount of sugar eliminated, and may be taken as affording a very good rough index as to the state of severity of the disease. The larger the quantity of sugar reaching and passing through the blood, the larger the quantity of fluid abstracted, in order to carry it away by the kidneys. I do not mean to say, that there is a rigorous relation between sugar and fluid, because the amount of the latter is likewise influenced by a variety of concurrent circumstances; but, that in general, a close relation exists is strikingly exemplified by reference to North's case (Case I, Appendix), in which, great fluctuations were occurring from the different diets administered. With only a very few exceptions, accompanying each rise and fall in the amount of sugar for the twenty-four hours, may be noted a rise and fall, in a similar direction, in the amount of urine. If a line were introduced into the plan belonging to the case (*vide* opposite p. 140), representing the daily amount of urine in drachms, it would be found to fluctuate throughout the greatest portion of its extent, in harmony with the line representing the daily amount of sugar. Indeed, there is only one point

at which the lines would be found to cross each other, namely, opposite Feb. 15th, when the diet consisted of jelly, meat, and beef tea. Here, there was a considerable descent in the amount of sugar without a corresponding fall in the quantity of urine.

Looking to the results obtained by recent analytical investigations, it is the *amount* of sugar which may be said to give to the urine its unnatural quality in Diabetes, and not the simple fact of *its presence;* for, sugar, it seems, must in future be ranked as a constituent—to an infinitesimal extent, it is true, of healthy urine. The higher the saturation of the urine with sugar, the greater of course is its deviation from the normal standard. The highest degree of saturation I have met with in my analyses has been 47 grs. to the fluid-ounce. In Case III, Appendix, the urine was restored by diet, from being charged to the extent of 44 and 46 grains to the ounce, to its natural quality—that is to say, restored to a state affording no reaction of sugar on being tested ordinarily with the cupro-potassic solution. In Case IV the lowest point at any time reached, was 2·96 grs. to the ounce, for the twenty-four hours' urine; and in Case I, the urine upon two occasions contained only a trace of sugar.

It was formerly stated, that urea was absent from the urine of Diabetics. Now, if such were a fact, the idea might be reasonably entertained, that sugar in some manner or another took the place—or rather, partially the place, on account of the presence of nitrogen in the one and its absence in the other—of urea. From more recent investigations, however, the statement is negatived, and suspicion must therefore be thrown over the correctness of former analyses. Urea, indeed, has even been frequently found to be excreted in larger quantities in Diabetes than under a condition of health. I believe it may be said, that urea and sugar have no direct connection of any sort with each other

in Diabetes. The excretion of urea will be influenced by the quantity and quality of the food, and the rate of destruction of the tissues; but I know of nothing to show, that it has any immediate or primary bearing in connection with the production of sugar.

The presence of sugar gives to the urine an increased density, and furnishes a character, which, combined with the increased quantity of the secretion, serves to afford a significant indication of the nature of the complaint. The usual range of sp. gr. of Diabetic urine may be stated as between 1030 and 1050, but it may be even higher than this; and, where the disease has been controlled by treatment, it may fall quite within the healthy standard: thus, upon one occasion, in North's case (*vide* in Appendix the urine passed between 9 a.m. and 1 p.m. February 15th) the sp. gr. fell to as low as 1010, and frequently was within a few degrees of this amount.

Generally speaking, the sp. gr. may be taken as varying with the extent of saturation with sugar. Such cannot, however, by any means be accepted as an undeviating occurrence; for, not unfrequently, there is very far from an accordance between the two. Referring again to North's case, a proof in point may be brought forward. On Feb. 8th the urine passed between 1 and 5 a.m. was of a sp. gr. 1032 and contained 23·05 grains of sugar per oz.; whilst the urine collected from 9 a.m. to 1 p.m. had a sp. gr. of 1023, but contained 24·40 grains of sugar to the oz. This is one example, but several of a parallel character might be quoted; so that the sp. gr. must not be looked upon as affording an absolutely safe criterion or reliable index, of the proportion of sugar that is present. As a rule, however, in accordance with general belief, the sp. gr. of the urine may be accepted as forming a pretty correct rough guide for estimating the degree of saccharine impregnation belonging to different specimens; it being always

borne in mind, that exceptions to the rule may not unfrequently occur, on account of there being other circumstances to influence the sp. gr. besides the presence of sugar. Under restriction, for instance, to animal food, where a large quantity is consumed, the sp. gr. of the urine may keep up, whilst the quantity of sugar has declined.

The colour of Diabetic urine will depend upon the amount of fluid that happens to be eliminated. In an ordinary case of Diabetes, where several pints of urine are passed in the twenty-four hours, and it is all mixed together; it appears almost as colourless as water. There is only a certain amount of colouring matter to be got rid of from the system; and this, being diffused through a large instead of a moderate bulk of fluid, creates the difference visible to the eye between healthy and Diabetic urine. Where the urine is collected in separate portions, for different periods during the twenty-four hours, it is found that the colour of the different specimens usually presents a considerable degree of variation. This is shown on reference to the tabular arrangement of results in North's case. Under a mixed diet, when any difference existed, as was the case in five out of the seven days that he was allowed ordinary mixed food, the specimen passed between one and five in the morning stood out prominently as to height of colour from the rest, and sometimes closely approached in appearance the character of healthy urine. Under a purely animal diet, by which the quantity of urine is materially diminished, the secretion may not be distinguishable by the eye from urine belonging to the natural state.

The clearness of Diabetic urine is another of its conspicuous characters. In the uncontrolled form of the disease, the urine never throws down any lithate deposit. Restriction, however, to an animal regimen alters the case. I have here seen a copious sediment subside, consisting of lithates. Such urine also often gives rise to the production

of crystals of lithic acid, especially after standing for some time; when, I expect, the acid generated by the lactic acid fermentation of sugar, liberates the lithic acid, which forms in crystals on the sides of the vessels, just in the same way as on the addition of an acid to healthy urine.

Several cases have been placed upon record, where an appearance of albumen has occurred in the urine during the existence of Diabetes. Probably, in some of these cases, the additional morbid element may have depended upon a structural disorganization of the kidney; but, I quite think that in others, the presence of the albumen may have resulted from a defective performance of the assimilative process, as regards this principle; just as I believe Diabetes to be caused by a similar occurrence as regards the saccharine principle. Facts might be mentioned, which prove, that an elimination of albumen may take place, owing to the form in which it is present in the blood. And, as saccharine urine can be occasioned artificially by puncturing a certain part of the *medulla oblongata*, so it has likewise been shown by Bernard, that albuminous urine has resulted, when the puncture has been made a little higher up. This strikingly suggests, that there may sometimes be a close connection, as to the mode of production of these two conditions. In an experiment of my own, many years ago; after puncturing the *medulla oblongata* of an animal to produce artificial Diabetes, I, not only, noticed the presence of sugar, but likewise of albumen. I cannot say, it is true, that the urine was not albuminous before the experiment; because, it did not happen to have been tested for that principle; but, there is every reason to believe that it was not so, the animal to all appearances having previously been in a perfectly healthy condition.

Next to the increase in the quantity of the urine, thirst is the most prominent and characteristic symptom of Dia-

betes. The insatiable thirst of which Diabetics so constantly complain is easily accounted for upon physiological principles. Sugar in its escape from the system through the renal organs, carries with it the water of the blood, and thus, is tending to alter the natural relation between the solids and fluids of the circulation. As soon as the proportion of fluid to solids, in the blood, is reduced below the normal standard, a sensation is excited, which induces in the animal a craving after that which is required to restore the balance to its proper point. Thirst like hunger is a warning provided by nature to indicate to us, when a certain requirement exists —a warning which if not responded to, becomes almost irresistible, from the distress that it occasions. It does not signify in what way the blood may become of too concentrated a character; whether, by the ingestion of a large quantity of saline matter as from eating salt provisions; whether, by an extensive elimination of fluid from the system; or whether, by abstinence from drinking: in each case, the result is the same, and a sensation of thirst is excited. In Diabetes, the strong desire that exists for fluids leads to a replacement of the water that is so largely carried away through the kidneys. The thirst of Diabetics, also, besides being occasioned by the large escape of liquid taking place, may likewise be partially dependent upon the physical presence of sugar in the blood, which must have the effect of increasing the density of the fluid.

By the extent of desire that exists for fluids, a pretty correct estimate may be arrived at, of the degree of severity of the complaint. Other circumstances being equal, it may be given, that the greater the quantity of sugar reaching the general circulation, the greater will be the fluid escaping through the kidneys, and the greater the thirst that is experienced.

Hunger is another usual accompaniment of Diabetes,

although sometimes, especially in the milder forms of the complaint, the appetite may not be excessive; and, even in the severer forms, it not unfrequently happens, that there may be, for a while, a complete loss of appetite, particularly towards an unfavorable close of the disease.

Hunger indicating the want of solids, just as thirst indicates that of fluids; it is not surprising that it should constitute, in general, a feature of Diabetes, and lead to the consumption of an excessive bulk of food, seeing how much of this food filters through the system as useless, instead of administering to the requirements of the economy, as under a condition of health. Besides this, however, there seems to be evidence—not unfrequently at least—of an excessive waste of tissue taking place in this complaint; and, a demand for food, which gives rise (from the sensation of hunger excited) to an increased consumption, may be created under this score. The appetite of some Diabetics is truly enormous, and unless frequently satisfied, the feeling that results, produces the greatest discomfort. It is not uncommon, for us to have Diabetics who are at all times ready to clear up everything left by their fellow-patients in the ward, and who express themselves as feeling when they sit down to dinner as if they could devour everything placed upon the table. The sensation has been described to me, as that of a sinking or emptiness at the pit of the stomach, which, even a full meal has not thoroughly removed.

The mouth is complained of as being dry, and the tongue is often clammy and covered on its surface with a white creamy fur or mucus; but at other times, it may be red and preternaturally clean.

Patients sometimes experience a sweet taste in the mouth; and in those, where the disease has been mitigated by treatment, this may be one of the signs by which they know when they have taken anything pre-

judicial to their complaint. The saliva not being saccharine, to produce this impression of sweetness, it has been suggested, that it should be looked upon as due to the existence of sugar in the blood directly affecting the nerves of taste. Magendie found on injecting bitter solutions into the veins of dogs, that the animals manifested the same signs of disgust as when the substance had been introduced directly into the mouth; and, it has been also observed, on substituting a relishable liquid, like beef tea, for the bitter infusion, as the injection, that the animals have licked their lips, as if perceiving an agreeable sensation. It has been suggested in these cases, as in that of Diabetics also, that the particular substance in the blood, on arriving at the capillaries of the mouth, influences the extremities of the gustatory nerves, just as if it had been placed in contact with the mucous surface.

The state of the intestine is exceedingly variable in Diabetes. In one patient, there may be a tendency to obstinate constipation; in another, to diarrhœa; whilst in a third, there may be no departure from what is natural. Sometimes, cases are attended, with constipation at one time, and diarrhœa most difficult to check at another. I have seen the inspection of a few cases in the hospital *post-mortem* room, where the occurrence of diarrhœa has occasioned such an amount of prostration as to have constituted the immediate cause of death; and I have sometimes thought, that the state of the intestine may have had some primary connection with the cause of the complaint.

There is a peculiar odour exhaled from the system, in the breath of the Diabetic patient, which has been differently described by different authorities. My own experience accords with that of Dr. Watson, and leads me to speak of it as more resembling the smell of ripe apples than that of anything else I am acquainted with. It, in fact, is

not unlike the odour evolved from the urine, and similar in character although less powerful, to what I have often perceived on testing for lactic acid in an animal product. According to Dr. Prout's description, the exhalation is compared to a sweetish hay-like odour.

The cutaneous function is not always influenced alike in this complaint. Certainly, the usual character that the skin presents is that of dryness and harshness. This is frequently spoken of as producing considerable discomfort. The cuticle on the palmar surface of the hand is unnaturally hard and stiff, and in the furrows produced by flexion, instead of a transparence, there is a white and mealy appearance. The cuticle, in fact, from its dryness, has lost its usual supple character, and shows a disposition to crack in the lines where flexion occurs. Sometimes, on the other hand, the patient may be frequently troubled with profuse perspirations. I have known Diabetics, even when the disease has been at its height, sweat most profusely. In one case that I particularly noticed a few years ago, I used often to find the patient, on my visit to the ward in the afternoon, lying upon his bed, with his face thickly covered with drops of perspiration. There was no pulmonary complication existing in this person.

The general appearance of a Diabetic is often quite sufficient to denote at once, to an experienced eye, the nature of his complaint, without the necessity of putting a single question. There is an emaciated condition of the body; but, besides this, there is a strikingly careworn, dejected, miserable, and pinched-up expression, so indicative of the despondent state of mind, and the constant bodily uneasiness or distress, belonging to the confirmed stage of the disease. The emaciated appearance may be in part due to the large abstraction of fluid that is constantly going on, but there is also a rapid waste of tissue taking place, so

that the patient loses in weight and strength as the disease advances. It is not to be wondered at, that such a deficiency of adipose tissue should be noticeable in the fully developed form of the complaint, seeing that the saccharine alimentary principle, which forms so large a bulk of our ordinary food, passes through the system and is lost, instead of contributing, as it ought to do, to the production of fat, and the maintenance of the animal heat. Proof is no longer needed to show that animals have the power of converting starch and sugar into fat. They become fat upon materials abounding in these principles. Deprive them of the power of elaborating or assimilating these substances, and one source of fat—a most important one, I believe—is immediately removed. Hence, kept upon an ordinary diet, it is not surprising, I say, that Diabetics should become so deprived of adipose tissue; because, they allow *that* to pass through the body and escape with the urine, which ought to be retained for its production.

Debility is a symptom that is always complained of, when the disease exists in an inveterate form. The patient experiences languor or lassitude, with an inability for sustained muscular exertion. There is usually, also, a dull pain spoken of, with a sense of fatigue in the back, loins, and lower extremities. Sometimes, these are the symptoms that are most dilated upon by the patient, in seeking advice, before the nature of his malady has been made known to him. Under such circumstances, his ailment may for the moment appear of the most indefinite description, until inquiries are made as to the state of the urinary function, when a clue is at once given, that leads to a proper diagnosis.

It would seem, that something beyond the waste of tissue is required to account for the debility that is experienced in Diabetes. It follows as a natural consequence, it is true, that when the muscular tissue has undergone a considerable

extent of atrophy, there cannot be the capacity for displaying an ordinary manifestation of power. The heart exhibits the same character that prevails throughout the other parts of the body, and thus, the weakness of the pulse that occurs. Now, although such a state of the muscular system may go towards accounting for the loss of muscular force noticeable in advanced cases; yet, the presence of sugar in the blood would appear to have something to do also, with this defective manifestation of power. North, when he had been kept some time on animal food, and had regained strength and vigour, complained, immediately he partook of a mixed diet, that he seemed to have no life, energy, or power about him. He could tell by his general feelings, when any article, or a particular kind of diet, produced a material increase in the extent of elimination of sugar. The explanation of this sudden change, appears to me to be, that the blood, when highly charged with sugar, ceases to hold a healthy position. It is well known, that for the efficient exercise of the various functions of life, a healthy state of the blood is required. Under any material deviation from the natural constitution of this fluid, it will cease, to a greater or less extent, to be favorably adapted for administering to its various purposes; and, the organs, as regards degree of efficiency of the work they perform, will be in default accordingly. For the muscular tissue to exercise its natural functional activity, a supply of blood, possessing a natural constitution, is demanded, as one of the conditions required. And, where a large amount of sugar is constantly passing through the circulation, as is the case in the inveterate form of Diabetes; such an alteration, I apprehend, is established, as may be taken satisfactorily to account for a deterioration in muscular strength.

What has just been said, may be accepted also, as applying to the loss of virility that is known to occur where the

Diabetic condition is intense. I look upon the altered constitution of the blood, as leading to the interruption of functional activity in this respect, and such a conclusion is strongly corroborated by a part of the history of one of the cases in the appendix. North (Case I), to whom I have before so frequently referred, gave an account of a loss of sexual feeling accompanying his complaint from its onset, until its intensity was subdued by restriction to an animal diet. A few days after he had been placed upon animal food, by which the quantity of sugar eliminated was most materially reduced; he confidentially mentioned to the reporter of his case, that he felt certain he was getting better, on account of a particular symptom he had observed. He had, according to his statement, been like a child hitherto during his illness, but now, he found that the nature of man had returned upon him. The restoration of the blood, to a closer approximation to its natural state, enabled, I presume, functional performances to be renewed, which had previously been held in abeyance.

There is a point in connection with Diabetes, which has recently excited considerable interest—I mean, the occurrence of cataract—on account of some experiments, which have lately been performed, showing that cataract may be produced artificially by the introduction of sugar into the system. Dr. Prout, in the fifth edition of his work on 'Stomach and Renal Diseases,' 1848, p. 32, thus refers, in the form of a note to the association of Diabetes with cataract: "As other instances of Diabetic derangement and "debility, I may state that I have seen two cases of the "disease accompanied by cataract. The one in a gentle-"man between fifty and sixty (lately dead); the other in a "young man between twenty and thirty. The young man "had been successfully operated on, and when I last saw "him, appeared likely to recover his sight."

Scarcely any attention seems to have been afterwards given to this matter, until Mr. France ('Ophthalmic Hospital Reports,' January, 1859, and 'Guy's Hospital Reports,' 1860 and 1861), placed upon record an array of cases, strongly tending to show, the existence of some immediate connection, between the occurrence of cataract and the presence of Diabetes. One of Mr. France's cases, was originally admitted into the clinical ward, under my own care, in July, 1860, and remained in my hands a short time, previous to being transferred to the ophthalmic department. The case was that of a woman aged thirty-four, who, at the time of admission was passing from four to six quarts of urine (sp. gr. 1040) a day, which in the course of a little more than a week became reduced to three and two pints only. Her impairment of vision had gradually taken place, and both eyes were affected alike. From Mr. France's examination, it was found, that there existed symmetrical, opalescent, lenticular cataracts (with superficial striæ) of unusual bulk. She could walk about the ward without assistance, and could distinguish large objects before her, but could not see sufficiently to make out their outlines. Her Diabetic complaint had been of about two years' duration, and about five months had elapsed, since she commenced to notice any affection of her sight.

The experimental evidence, which has been recently brought forward, on the opposite side of the Atlantic, harmonises with the inference that had already been drawn from practical observation; and, beyond all question, shows how immediately connected the occurrence of a cataractous condition of the eyes is, with a saccharine state of the system. Dr. S. Weir Mitchell ('On the production of Cataract in Frogs by the administration of Sugar'—'American Journal of Medical Science,' January, 1860), whilst per-

forming experiments upon the osmosis of woorara through animal membranes, incidentally found, that the injection of a certain amount of syrup underneath the skin of frogs, occasioned death within a few hours; and, at the same time, produced a white appearance of the eyes, which proved to depend upon cataract. Subsequent experiments were conducted, to ascertain the manner in which the effect upon the eyes was produced. I will give an extract of one of the experiments related, which will speak for itself on the subject.

"*Experiment.*—About two drachms of syrup were in-" jected under the skin of a large frog. In twenty-four " hours the lens was opaque, and, as the animal appeared " lively, it was placed in water, in order to test the perma-" nence of the opacity. Ten hours in the water sufficed " to remove most of the opacity from the lens, which began " to clear in the centre first. Twenty-four hours after the " frog had been placed in water, the eyes were perfectly " transparent, and the animal itself entirely well."

Dr. Mitchell also observed that the same effect could be produced by soaking the eyes of frogs, or the lenses themselves, in a solution of sugar; and, that when cataract was occasioned, it disappeared on placing the lenses in water. The conclusions, he has summed up with, are:—"*First.* That " sugar in large amounts destroys the life of the frog when " given internally, injected under the skin, or thrown into " the stomach. *Second.* That an abundant supply of water " frequently enables the frog to eliminate the sugar and " escape death. *Third.* That the formation of a peculiar " variety of cataract is one of the most curious and striking " symptoms attendant upon the sugar poisoning. *Fourth.* " That the cataract is due to mechanical disturbances of " the form and relative position and contents of the com-" ponent tubes of the lens."

Dr. Richardson ("The Synthesis of Cataract."—'Journal de la Physiologie, par E. Brown-Séquard,' 1860), has repeated and confirmed Dr. Mitchell's experiments. He has also tried the effect of a great variety of other solutions, and has found them to act in the same way as syrup, with the exception of the iodide of potassium, which left the lens unaffected in every case. In his summary, he says: "In "the first place it is to be observed that the success of the "experiment in producing the cataractous condition turns "on the specific gravity of the fluid injected. It required "in every example that the specific gravity should exceed "1045, in other words that it should exceed the specific "gravity of the blood. But so soon as a condition of the "blood was obtained, so soon as the circulating fluid could "afford secretions, having an abnormal density, then the "cataractous change was induced, and lasted so long as the "blood retained its abnormal state."

I regard these experiments on the artificial production of cataract, as of great interest, on account of their showing, the manner in which morbid results may arise in concurrence with Diabetes. I have before given it as my opinion, that the affection of the lungs, and other complications, attendant upon Diabetes, depended upon the presence of sugar in the blood producing such a deviation from its natural character, as to interfere with the conditions required for the healthy performance of the nutritive and other operations of life. These experiments on the eye, give a strong aspect of probability, to an opinion founded entirely upon other considerations. The physical alteration of the blood, caused by the presence of a certain amount of sugar, disturbs its natural position in relation to the structure of the lens, so as to lead to the production of an abnormal condition of the latter. From the same kind of disturbance, morbid occurrences may arise in connection with other

structures. The deviation from the healthy standard of relation between blood and texture is such, that a very trifling cause may be sufficient to disturb the balance directed towards healthy nutrition; and so, lead to the occurrence of congestion and inflammation.

In the treatment of Diabetes, as there has been nothing yet made known, which enjoys the property of effecting a direct cure of the complaint in its inveterate form; all that we can suceeed in performing at present is, to produce a palliation of its symptoms and its severity. Happily, I believe, the cases are very few, in which the measures at our command, will fail in enabling the patient to live in a fair amount of comfort and security.

The disease in its recognisable result, consists of a defective power of assimilation, over one of the alimentary principles we consume. And, I earnestly echo the opinion of Dr. Prout that "the first and chief point to be attended "to in the treatment of Diabetes is diet." With a healthy person, when saccharine and farinaceous materials are ingested, as in the form of bread and many other vegetable articles of food, we lose sight of them in the system; because, there is an aptitude for assimilating them, so that, they become serviceable for administration towards the requirements of life. Their physiological destination fulfilled, their elements, it may be fairly presumed, escape chiefly in the form of carbonic acid and water from the body. With the Diabetic, however, such kind of food, instead of being appropriated, is allowed to filter through the system undecomposed. A morsel of bread being eaten, the starch is converted into sugar, but the transformation proceeds no further. There exists the incapacity for carrying out any subsequent elaboration; and, as the consequence, the sugar reaching and circulating with the blood, is pumped off,

with its elementary constitution unchanged, by the kidneys; producing symptoms of constitutional derangement in its passage through the system.

The object, it must be admitted by all, to be kept in view in the treatment of Diabetes, is to place the patient in a state as closely approximating the standard of health as possible. In no complaint, have we, I believe, so plain an indication before us, of the precise nature of the end to be attained as in this. In Diabetes, *par excellence*, the management of the case is open to rational principle for its guidance. An element of our food, from a want of being properly rendered susceptible of disposal, makes its transit through the body by means of the circulation, and thus, gives to the blood a deviation from its natural condition. When it is taken into consideration, what an enormous quantity of sugar may be voided in Diabetes (in Case II, Appendix, my examination one day gave me upwards of nineteen thousand grains, in fact, rather over $2\frac{3}{4}$ lbs. for the twenty-four hours) it is evident, how great must be the alteration, in the state of the blood, from that which is normal; for such an amount of sugar, in such a space of time, to pass through it. To reduce the extent of alteration, to its minimum degree possible, is, I hold, the point that should be sought to be attained by treatment. No one will deny, the desirability of placing the circulating fluid in as physiological a state as possible, in order that it may be in a position healthily to administer to the functional operations of life.

To abstain from the particular element of food, which fails in the Diabetic to be assimilated, forms the rational course of procedure to accomplish the object that has been mentioned. And, happily for the patient, the withdrawal of this alimentary principle does not in itself entail any injurious consequences. There are those, dwelling in some

parts of the globe, who habitually subsist without it. Its place can be supplied by another principle, differing in character, but having the same physiological destination; and, connected with which, there is no defective power of appropriation apparent in Diabetes.

Restriction from saccharine and farinaceous articles of food, in some cases, temporarily eradicates every symptom of the complaint. I have already referred to cases of this description, that have fallen under my own observation. Bouchardat, as far back as 1841, noticed indeed this occurrence, and placed upon record, in the 'Comptes Rendus de l'Academie des Sciences,' two cases where such an effect had been produced. Mr. Camplin, who has kept his disease at bay for ten years, as remarked by Dr. Parkes, appears to have suffered only from this form of the disease, viz., the sugar owning no origin but that directly due to the food.

In the common form of the complaint, although the restriction in diet palliates the symptoms, by materially reducing the amount of sugar for escape; yet, the sugar never entirely disappears from the urine. Notwithstanding, however, that the sugar cannot be made to disappear, yet its reduction in amount, I feel most strongly, places the patient in a position, not only, of much greater comfort, but likewise of much greater security, than he otherwise would be. A Diabetic, who is voiding a large quantity of sugar, and continues unrestricted in diet, dwells among us, with but a very insecure hold upon life. With the constitution of the circulating fluid, so altered by the presence of sugar passing in such quantity through it, very slight causes are sufficient to set up disturbances which may prove of the most serious character to the patient. A great many circumstances, which would lead only to a trivial and temporary derangement in a healthy person, are observed in

the intensely marked form of Diabetes, to occasion even a fatal result. From what I have seen upon looking around, I am strongly of opinion, that when, by dietetic measures, the elimination of sugar is kept down; the patient is not in the same way liable, to those accidental complications, which so frequently produce a fatal termination in Diabetes. I do not go so far as to say, that the restriction in diet will in every case prevent Diabetes from proving fatal; because, it would seem, that a form of Diabetes may occur, with some other circumstance in operation, to produce a gradual failing of the health, besides the mere functional aberration as to saccharine assimilation; but, I do think, that the secondary ailments may be, in a great measure, if not entirely, staved off by it. I quite concede, that after a Diabetic has had his complaint for some time, his system having become habituated to the unnatural condition, he may experience but little personal inconvenience upon an unrestricted diet, although he is voiding an enormous quantity of sugar every day. As far as I am informed, however, by what I have heard of, and seen; such a person is living in comparative and unnecessary insecurity. He is exposed to the occurrence of incidental complaints, of a fatal character; which a closer approximation to the healthy state, as regards constitution of his blood, might have enabled him to escape.

The influence that diet is known to exercise over the elimination of sugar in Diabetes, is very far from a matter of recent discovery. The effect of different régimes, in augmenting or diminishing the quantity of sugar in the urine, has been long since decided by analytical examination. Still, it occurred to me that it would not be a work altogether of supererogation, to take a suitable patient in hand, and rigidly submit his case to investigation; by examining, for many days consecutively, the urine belonging to sepa-

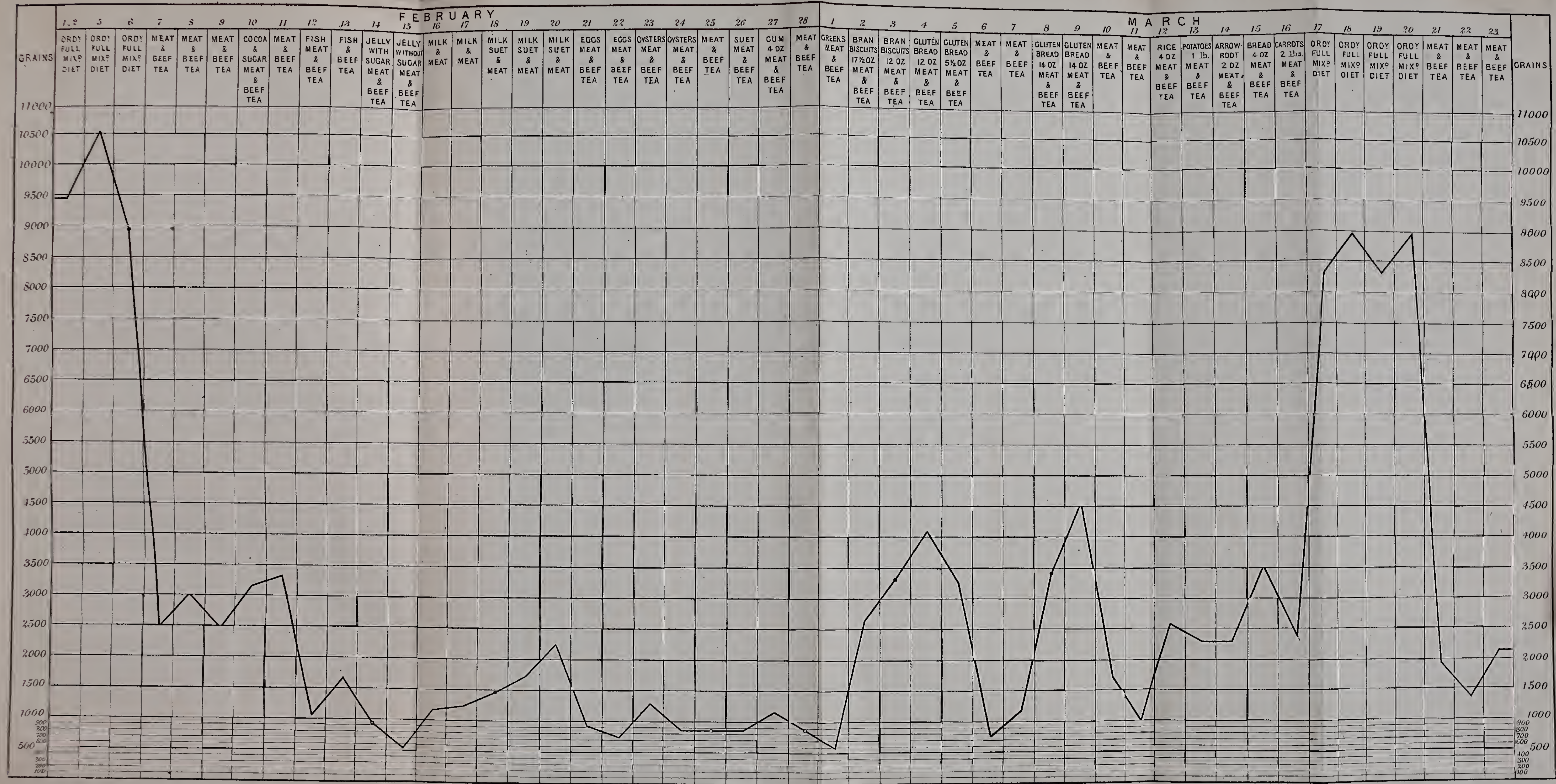
rate periods of the twenty-four hours, under the administration of various articles of food.

North's case was, therefore, undertaken, a full report of which, is to be found in the Appendix. The twenty-four hours were divided into six periods, consisting of four hours each. The urine belonging to each period, was separately collected and examined, and this proceeding was continued, from the commencement of February, till towards the end of March; during which time, many different articles of diet were administered, and their effect on the elimination of sugar determined. I do not wish to over-estimate the value of labour that has been bestowed by myself; but, I believe the report of this case will be found, on perusal, to present features of considerable interest.

All the analyses that are given, were from day to day made by myself, and the patient was under the most strict supervision. Scrupulous care was taken, to obtain the utmost precision that was possible; and, I can confidently advance the report, as a truthful record throughout. The Appendix contains the several results obtained, placed in a tabular form; besides, a résumé of the daily occurrences that were observed.

The results supplied by this case, have also been arranged, so as to give at a glance, a view of the effect produced by different articles of food upon the patient, as regards the elimination of sugar. The accompanying plan, which I have thought it desirable to introduce here, instead of in the Appendix, needs but little explanation. The fluctuating line represents the variation in the sugar passed, the daily amount of which, in grains, can be read off from the scale on either side. For each day, the nature of the diet is given, so that the comparative effect of the various articles of food administered, is at once apparent, on referring to the position of the line. The quantity of the food, where

PLAN SHEWING THE VARIATION IN THE DAILY AMOUNT OF SUGAR PASSED UNDER DIFFERENT DIETS BY JOSEPH NORTH, ÆT. 32, A DIABETIC.



London. Day & Son Lithrs to the Queen,

this is not stated, can be learnt on referring to the details contained in the Appendix.

The rapidity with which the effect of food manifests itself upon the urine, is shown upon several occasions in North's case. February 10th, when a purely animal diet had been adhered to for a few days, he departed from his instructions; and, with his tea at 4 p.m. partook of some prepared cocoa sweetened with sugar, which his friends, during the afternoon, had brought him in. During the four hours between 1 and 5 p.m., the sugar voided was 468 grains. During the succeeding four hours the quantity rose to 1311 grains, and for the next four hours descended again to 483 grains. March 12th, having again been for a couple of days previously, on a purely animal diet, he was ordered some rice. At 6 a.m. he ate for breakfast, half a rice pudding composed of 4 oz. of rice and 1 egg. From 1 to 5 a.m., the quantity of sugar passed had been 81 grains; whilst from 5 to 9 a.m. it amounted to 576 grains. On the following day, again, he was allowed for breakfast at 6 a.m. 1 lb. of potatoes without anything else. From 1 to 5 a.m. the sugar eliminated was 281 grains, and from 5 to 9 a.m. 794 grains.

The above examples strikingly exhibit, how rapidly, sugar derived from the food consumed, filters through the system and appears in the urine. Some time past, Mr. G. N. Bacon made, in accordance with my suggestion, some observations on the immediate effect of a meal upon the amount of sugar in the urine. The case submitted to observation, was that of a lad seventeen years of age, whilst an inmate of the Norfolk and Norwich Hospital, under the care of Dr. Ranking. The following are the results that were obtained:—

Observations on the urine secreted, 4 hours before, and 4 hours after, breakfast; with an allowance of wheaten bread *ad libitum*, milk, tea and sugar.

		Quantity of urine.	Quantity of sugar.	Proportion of sugar to fluid-ounce.
		oz.	grains.	grains.
Jan. 6th.	4 hours before breakfast	22	575	26·
	4 hours after breakfast	28	1344	48·
,, 7th.	4 hours before breakfast	18	432	24·
	4 hours after breakfast	21	1008	48·
,, 13th.	4 hours before breakfast	12	345	28·8
	4 hours after breakfast	23	1104	48·

Observations on the urine secreted, 4 hours before, and 4 hours after, breakfast; consisting of gluten bread, milk and tea. (Here the difference before and after the meal is not so great as in the preceding series, on account of the smaller quantity of saccharine and farinaceous principles ingested.)

		Quantity of urine.	Quantity of sugar.	Proportion of sugar to fluid-ounce.
		oz.	grains.	grains.
Dec. 30th.	4 hours before breakfast	26	439	16·92
	4 hours after breakfast	20	720	36·
Jan. 1st.	4 hours before breakfast	22	288	24·
	4 hours after breakfast	20	822	41·10
,, 2nd.	4 hours before breakfast	16	384	24·
	4 hours after breakfast	24	986	41·10

Observations on the urine secreted, 4 hours before, and 4 hours after, dinner; consisting of meat, gluten bread, watercress, and brandy and water. (Here it must be

taken into consideration that the urine before the meal was under the influence of a preceding one).

		Quantity of urine. oz.	Quantity of sugar. grains.	Proportion of sugar to fluid-ounce. grains.
Dec. 14th.	4 hours before dinner	20	639	31·98
	4 hours after dinner	24	864	36·
,, 15th.	4 hours before dinner	16	576	36·
	4 hours after dinner	26	936	36·
,, 17th.	4 hours before dinner	20	822	41·10
	4 hours after dinner	24	767	31·98

The information supplied by Case III Appendix, would tend to show that, not only, do the starch and sugar ingested, traverse the system unappropriated; but, that the presence of sugar in the system, acts prejudicially upon the condition belonging to the complaint, and gives rise to the elimination of more sugar than could be derived from the materials themselves taken. Thus, after restriction to an animal regimen had been observed, and sugar had for some days entirely disappeared from the urine, two ounces of ordinary bread a day, for a couple of days, were allowed, and then the animal diet again resumed. During the first day, the patient's urine contained 117 grs. of sugar; the second, 196 grs.; the third, 213 grs.; the fourth, 571 grs.; the fifth, 236 grs.; the sixth, 92 grs.; and after this, the presence of sugar was lost. From the four ounces of bread taken, evidence of the elimination of 1425 grs. of sugar with the urine, was thus obtained; which, is more than what could come directly from the bread ingested. I find that two ounces of the hospital bread, when perfectly dried in a water oven, left a residue weighing 600 grs. The amount of the whole of the solid matter in the bread consumed

was, therefore, only 1200 grs., against the 1425 grs. of sugar detected in the urine. Another point worthy of note, is, that the elimination of sugar continued increasing in amount for two days after the bread had been discontinued; and, that it was not until after the sixth day from the commencement, that the urine regained its pre-existing character.

A strict exclusion from saccharine and farinaceous materials, which implies almost a total abstinence from the ordinary vegetable food, has been warmly advocated by many, during several years past, in the management of Diabetes. Physiologically, an animal regimen contains all that is necessary for maintaining human life in a state of health. But, there can be little doubt, looking to the teeth, the articulation of the lower jaw, and the general inclination of man, that he is designed by nature as an omnivorous feeder; although, his capacity for adapting himself to variations in external circumstances, is well known to be sufficiently extensive, to enable him to confine himself to either an animal or a vegetable regimen.

Thus, there are those, even amongst Europeans, who from choice restrict themselves to vegetable food, and we do not find that the vegetarian mode of living leads to defective nutrition.

There are those, on the other hand, who from necessity subsist on animal food alone. The Arctic Highlander is obliged to restrict himself to an animal diet, because he can procure no other. It wisely happens, that this animal food, abounding in fat as it does, is the best suited to the circumstances under which these northern inhabitants of our globe are placed; but, independently of this, the character of their climate, altogether precludes the growth of the vegetable products, which enter into the mixed diet of other nations. From the account of an exploration of Baffin's

Bay given by a celebrated northern voyager, we are told that the most conspicuous vegetable productions of the Arctic Highlands consist of heath, moss, and various coarse grasses. There was no appearance of cultivation, nor was it discovered that the natives made use of vegetable food.

Besides those who practise restriction to an animal diet through necessity, there are others who adopt it from some other cause. Dr. Carpenter in his work on the 'Principles of Human Physiology,' 4th edition, p. 379-80, says, "There are particular conditions of existence, however, "under which life may be advantageously supported upon "*animal* food alone. Thus the Guachos of South America, "who pass the whole day in the saddle, and lead a life of "constant activity resembling that of a carnivorous animal, "scarcely ever taste anything but beef; and of this their "consumption is by no means great; for the temperature "of the surrounding atmosphere is so high, that the body "has no occasion to generate more heat than is supplied "by the combustion of the hydro-carbonaceous portion of "the 'waste' of the tissues."

There can be no reason, therefore, against a strictly animal regimen being adequate to all the physiological requirements of the Diabetic; and, I believe, if persevered in for a short time, it would come to be taken without giving rise to any feeling of material privation. Indeed, I have been told by patients, whom I have restricted to animal food; that, after a little while, they have experienced no desire to take any other. At first, however, the inconvenience cannot fail to be considerable. Independently of the natural desire that exists to continue with what we have been accustomed to all our lives; the increased appetite of the Diabetic, has led him into the habit of consuming a large bulk of food, and, when an abstinence

from vegetable materials is at first observed, he finds it difficult to satiate his hunger. His stomach has been accustomed to be largely filled, and he feels now that he wants a greater bulk of material than what he can conveniently get with animal food.

In commencing an animal regimen, it is desirable therefore, to allow a much larger quantity than will be afterwards required. In a very short time, as the drain from the system diminishes, the appetite will present but little above an ordinary character. Nothing so thoroughly relieves the inordinate appetite and thirst, which form usually such symptoms of discomfort in Diabetes, as diminishing the elimination of sugar by restriction from saccharine and amylaceous articles of food. The administration of opium in one-grain doses, two or three times a day, will be found to conduce towards alleviating the sense of emptiness at the stomach when the animal diet is first prescribed.

It may be as well to remark, for those who look to the sp. gr. of the urine as affording an indication of the progress of the case, that allowance must be made for the density being naturally high under an animal regimen. It is a well-known fact, that the ingestion of a large quantity of animal food notably increases the amount of solids eliminated with the urine. The sp. gr., therefore, may remain comparatively high, notwithstanding there has been a material reduction in the sugar.

Although it must be always admitted, that so close a restriction in diet, cannot be likely to do otherwise than prove more or less irksome, particularly at times; yet, I should imagine the distress belonging to the inveterate and uncontrolled form of the complaint was incomparably worse to endure. But, attention has been directed towards providing for the Diabetic some substitute for ordinary bread, which he may take, without prejudice to his disease, with

his animal food. There are three materials, which, in this light, I have to notice here. Two of them have long been known, and are derived from different portions of the wheat grain; the other, has been recently introduced by myself, and is derived from quite a different kind of vegetable production.

In the 'Comptes Rendus de l'Académie des Sciences,' Nov., 1841, p. 942, there is a communication by Bouchardat entitled, 'Nouvelles recherches sur le diabète sucré ou glucosurie,' in which he suggested the administration of gluten bread, and gave cases to show the advantage of its employment. I need not say that the gluten bread is now extensively known and consumed. As supplied by Mr. Van Abbot,* from importation from France, it is a most light and elegant looking article. I have often given it a trial, and can bear testimony to the great superiority it enjoys over ordinary bread, in its effect upon the urine of Diabetics.

Gluten bread is not without its objections, however. Although, some patients eat it very well; yet, others complain, that when they get it into the mouth, it seems as if they were chewing india-rubber. But, it is not likely that any article, as a substitute for what we have been so long accustomed to, will give satisfaction to all. Another objection is the presence of starch which has been imperfectly washed away in the process of preparation. I do not know whether or not the gluten bread is so carefully prepared as it was some years ago; but what I have tested recently has been charged to so free an extent with starch as to have become almost black upon being treated with a solution of iodine. In North's case, as seen by the plan, the gluten bread does not stand in so favorable a position,

* Mr. Van Abbot's address is 148½, Fenchurch Street, London. Mr Blatchley also prepares a gluten biscuit.

as regards effect upon the urine, as the bran biscuit. Gluten bread was given upon two occasions, on purpose to corroborate the first comparison.

For the manufacture of gluten bread, it is from the interior of the grain that the product is obtained. The exterior, or bran, has likewise been prepared and recommended as a suitable food for the Diabetic. Dr. Prout* says, "For some time past, I have recommended as a sub-"stitute for bread in Diabetes, a compound of bran, eggs, "and milk, which if properly prepared is not unpalatable." He further states, that a patient to whom this bran-bread was recommended, took much pains to perfect the process, and then describes what had been found to be the best plan to be adopted. Dr. Camplin, from personal experience, speaks very strongly in favour of the bran food as a substitute for ordinary bread, and through him a great improvement has been made in its manufacture. Dr. Prout directed that the bran should be washed, to remove adherent starch; but his product must have been left in a very coarse state. Dr. Camplin has specially turned his attention to the reduction of the bran to a finely divided state, so that it may be more readily acted on during digestion. He has found that after being well washed and dried, being thereby rendered friable, it can be ground to a powder (in a mill specially contrived and sold for the purpose) of a degree of fineness that could not have been reached before.

At the best, however, the bran food cannot be looked upon as otherwise than a dry and unrelishable article for consumption. Biscuits are sold by Mr. Smith, of Gower Street North; and Mr. Blatchley, of 362, Oxford Street. The latter, also, sells the powder, preparing it according to Dr. Camplin, for those at a distance, who have no mill or

* 'On Stomach and Renal Diseases.' Fifth edition, p. 44.

wish to avoid the trouble. Smith's biscuits are those that have been used at Guy's Hospital.

The hardness of the bran biscuit not unfrequently renders it impossible for patients to take it. It is quite a common grievance with Diabetics to have loose and very unsound teeth, or scarcely any teeth at all. And, I have upon several occasions fallen upon cases, where the biscuit could not be consumed from this cause. Mr. Blatchley, however, has met this emergency by preparing a softer kind of biscuit, which he calls in contradistinction bran cake.

After the process to which the bran has been submitted before grinding to a powder, it may be reasonably doubted if much, or, indeed, scarcely anything remains of a nutritive value. All acknowledge that the covering of the wheat contains a larger proportion of tissue-forming material than the central farinaceous part. And, as we choose to have whiteness as a prominent quality of bread, we therefore reject, as far as our own consumption is concerned, the most nutritive part of the grain. But, it is the inner part of the integument, that is thus rich in nitrogenized principle; and, being more brittle or mealy than quite the outside husk, it will be more finely reduced when the corn is ground, and thus afterwards, in great part, go with the pollard in the process of dressing. The very outside covering, being of a coriaceous description, separates in scales, and forms the chief bulk of bran. Consisting, as it does, of a form of lignin, it is unsusceptible of digestion in the human alimentary tract; and, therefore, useless as far as the nutritive and calorifacient processes of life are concerned. It will certainly play the part of assisting to give bulk to what is consumed, and so enable the person to satisfy his hunger by filling the stomach; but, there are some patients with whom the presence of an indigestible material like bran, in the alimentary tract, creates such irritation that it

cannot be borne. It is not very uncommon for Diabetics to complain that they are unable continue the bran biscuit, on account of the diarrhœa that has supervened. On the other hand, however, it often happens, that the ingestion of a material possessing slightly irritating properties is desirable, in order to stimulate the muscular and mucous coats of the intestine and overcome the constipation that is frequently incident to the complaint.

Seeing the objections that existed to the present substitutes for ordinary bread, at the command of the Diabetic, I began to look around at the various vegetable products with which we are supplied, to endeavour to find out if something more suitable could not be provided. Attention had been hitherto confined to the separation of the objectionable principle—starch; from the other constituents of the grain (gluten on the one hand and lignin on the other) in use amongst us for the manufacture of our ordinary bread. That this separation, however, must be carried out but imperfectly in practice is prominently shown by the plan belonging to North's case, introduced opposite p. 140. And, harmonising with these results obtained upon North, Abbot's gluten bread, is, as I have previously mentioned, found upon testing, to give a free indication of the presence of starch. So, also, Smith's and Blatchley's bran biscuit; and, Blatchley's bran cake, bran powder, and gluten biscuit, each produce a deep-blue coloration with iodine indicative of starch.

It occurred to me, to discard from consideration, the cereal grains and look to the seeds that are easily to be procured at a moderate expense; where, instead of starch, oil exists. Of course, freedom from any strong taste and an absence of any deleterious principle formed absolutely essential conditions to be attended to. Suffice it to say, after looking around, I alighted upon the sweet

almond, as the most promising for my purpose. The hemp seed in constitution much resembles the almond, and would probably produce a less expensive material, but there are difficulties in the way, in getting the kernel free from its coverings, that rendered it, I thought, desirable to try first what could be done with the almond.

In the first place, I ascertained that the almond could be procured in quantity at a moderate price. The Barbary and Sicilian, which are the almonds used by confectioners, are sold, deprived of shell, at sevenpence to eightpence per pound. I also inferred, from the fact that animals are fattened with the cake left as a residue after the extraction of the oil, that the almond might be fairly expected to contain nothing to prevent its prolonged use,, or its being resorted to as a staple article of food. Common observation supplies evidence that the occasional use of this seed by ourselves does not give rise to any prejudicial consequences.

The following is the composition of the sweet almond, that I find given in Dr. Pereira's work on 'Food and Diet:'—

Boullay's Analysis of Sweet Almonds.

Fixed oil	54·0
Emulsin	24·0
Liquid sugar	6·0
Gum	3·0
Seed coats	5·0
Woody fibre	4·0
Water	3·5
Acetic acid and loss	0·5
Sweet almonds	100·0

Dr. Pereira says,* "Sweet almonds are nutritive and

* 'A Treatise on Food and Diet. By Jonathan Pereira, M.D.' 1843, p. 340.

"emollient; but, on account of their fixed oil, difficult of "digestion, at least when taken in large quantities, or by "persons whose digestive powers are weak." With this expression, the opinion of most will fully coincide; and, it equally applies to the hazel-nut and filbert, walnut, brazil-nut, cocoa-nut, in fact to all the oily seeds. I quite agree that these productions, in their ordinary state, should be ranked amongst articles of difficult digestibility; but the cause is easily explained, and the difficulty, likewise, as easily overcome.

For the digestion of a substance to be accomplished, it is necessary that the secretions of the stomach should be capable of penetrating and coming in contact with its particles. Taking a given article, the facility or rapidity with which it can be digested, will depend upon the freedom with which it can be intimately incorporated with the gastric juice. Hence, the assistance to digestion afforded by the preliminary process of mastication. Now, with one of the oily seeds, it is not likely that mastication can be so perfect as to reduce the whole to a uniform pulp or paste—such as would be easily penetrated throughout by the digestive fluid. Small uncrushed pieces are swallowed; and these, being dense and oily, resist penetration by the solvent medium; which, therefore, is placed at a great mechanical disadvantage for exercising its properties.

I was struck some time ago, whilst conducting some experiments on artificial digestion, to find the white of the hard-boiled egg more readily acted upon than the yolk. The presence of oily matter in the yolk, as in the case of the oily seed, by offering obstruction to the penetration of the solvent fluid, will interfere with the accomplishment of the digestive process.

From these considerations, the obvious means of removing any objection to the use of almonds as a daily article of food,

on the score of indigestibility, will be, to have them thoroughly ground as a preliminary process, to being presented in a form for consumption. When this has been accomplished, I am strongly of opinion, that no inconvenience is likely to be experienced from the want of facility of undergoing the accustomed disposal effected by the stomach.

The oily character of the almond is a desirable quality for it to possess, as an article of extensive consumption in Diabetes. The Diabetic fails to be able to make use of one form of the respiratory or calorifacient element of food, but with the other—the oleaginous—there exists no difficulty. Theoretically, then, the Diabetic ought to be supplied pretty freely with fat; and, practically, it is found that such a supply is highly beneficial. Dr. Prout, in a note, from which I will give an extract, at p. 40 of his work on 'Stomach and Renal Diseases,' 5th ed., says, "We may observe here, that "oleaginous matters often agree so remarkably well in "Diabetes, that some have gone so far as to propose them "as remedies. When freely taken, they usually cause a "flow of saliva, and thus diminish the urgent thirst. When "they agree, also, they give a sensation of satisfaction and "support to the stomach, which other alimentary sub-"stances do not." It may be inferred that this last expression has come directly from the lips of a Diabetic, as it so exactly accords in force with a statement made by North (Case I, Appendix) after he had been for three days partaking pretty largely of suet. His expression was, that he liked the suet, and that it filled up the void which seemed to exist at the pit of the stomach, more than anything else he had yet taken. It was evident that the impression thus referred to, resulting from the ingestion of fat, must be very marked and decided; for it to have dictated, through these different channels, words so expressive of identically the same meaning.

Chemically speaking, then, it would seem that the almond, with its 54 per cent. of oil in the place of the starch of the cerealia, is admirably adapted to supply to the Diabetic a substitute for the bread of ordinary use, which is, from the nature of his complaint, prejudicial for him to consume. From the large proportion of the nitrogenous element which the almond also contains—viz., 24 per cent., it forms in addition, a material richly endowed with nutritive properties. It is not quite free from objectionable constituents; containing, according to the analysis given, 6 per cent. of sugar and 3 per cent. of gum. The sugar exists in the form of cane sugar, and to become recognisable to the indication of our copper test, must be converted, before testing, into grape-sugar; which is easily effected by boiling for a short time with a few drops of dilute sulphuric acid. There is not a trace of starch discoverably present.

The sugar and gum are easily separated, thereby, leaving a material as unexceptionable as animal food. In effecting this separation, it is of course desirable that nothing else should be removed, as such would constitute a loss. The nitrogenous matter spoken of as emulsin, enables water to take up more or less of the oleaginous principle, thus forming an emulsion, or in fact an almond milk. The nitrogenous matter of the almond holds a position identically analogous in this respect, to the caseine of animal milk. Now, the object clearly before me, in getting rid of the sugar and gum, through the agency of water, was to avoid the production of an emulsion. And, I soon found that the retention of the oily matter was to be thoroughly effected, by keeping back or rendering insoluble the nitrogenous matter. Without the presence of this latter in solution, the oily matter cannot be taken up, or an emulsion be formed, from the almond. On boiling the ordinary emulsion, a considerable coagulum is formed; but still, the liquid remains

milky, although in a much less degree than before. On the subsequent addition of a small quantity of acid, another coagulum is formed, and now, the liquid is rendered clear. In fact, the almond contains, both albumen and caseine, and to coagulate or render the two insoluble, an acidulated boiling liquid must be used. The process I have found to answer the best, and have recommended to be adopted, is to pour boiling water, slightly acidified with the tartaric acid, over the almond powder. This washing effectually carries away the sugar; and the liquid that comes away is nearly as transparent as water, and devoid of the principles that it is desired should remain.

Thus far, I have spoken of the almond in reference to its theoretical adaptability as a food for Diabetics. I come now to the practical side of the question. Finding that with eggs a solid form of biscuit could be procured, I sought the assistance of Mr. W. Hill, of 60 and 61, Bishopsgate Street, E.C., London, an expert confectioner. To Mr. Hill, jun. my thanks are due for having entered so fully as he has done into the spirit of the undertaking. Several difficulties had to be overcome. And, many trials of different plans were effected, the object held in view being to produce a material as closely resembling our ordinary wheaten food as possible. The only substances taken for use, are, blanched almond powder, which has been washed, and eggs; so that, the product is free from any objectionable principle.

It is not for me to speak in praise of this almond food, myself; but, thus much I may say, that Mr. Hill has succeeded in producing a rusk and different forms of biscuit, which give me good cause to hope that the Diabetic may find in this application of the almond a not unimportant nor unpalatable accession to the limited list of articles he is permitted to consume, without producing an aggravation of his complaint. And, there is this in the almond food, that the

patient is supplied in his substitute for bread, with that which he can appropriate, and which his system is exactly in need of. It must not, of course, be expected that an article equally as relishable as bread can be produced. Growing up, as we do, from almost our earliest period of life, upon wheaten food, it is not likely that its place, certainly at first or until a second habit has been engendered, can be supplied with anything that will prove so agreeable for a continuance.

The almond rusk and biscuits manufactured by Mr. Hill come within the range of price of the other substitutes for bread in use, and, being dry, they will keep for a length of time; and so, may be sent to any distant part. A loaf has been produced which is sufficiently light and not bad eating, beyond a retention of moistness which we have not yet succeeded in overcoming. There happens to be nothing in the materials used, of an absorbent or hygrometric character like starch. Possibly, however, some other substance in the course of time may be found to add, which will supply the desideratum without in itself proving objectionable. Indeed, the application of the almond to the present purpose has been put in force so short a time, that there is little doubt considerable improvement may yet be made.

I do not come forward to advocate the use of this food, without having had practical hospital experience, such, as the limited time it has been at my command has permitted, of its effects in the disease. I gave it in two cases and found no increase in the quantity and sp. gr. of the urine, over that which occurred under a strictly animal regimen. But, I have also given it in two other cases, where a daily analytical examination of the urine was made. These I will introduce and leave to speak for themselves.

David J.—, æt. 48. Diabetes of several months' duration, unattended with any other complaint.

Date.	Quantity of urine.	Sp. gr.	Proportion of sugar to fluid-ounce.	Quantity of sugar per 24 hours.	Diet.
	oz.		gr.	gr.	
Jan. 15	144	1043	38·70	5572	Full mixed diet.
„ 16	144½	1042	38 70	5592	
„ 17	146	1042	42 85	6256	
„ 18	156	1041	37·50	5850	
„ 19	120	1043	37·50	4500	
„ 20	49	1013	30·	1470	Animal diet, with greens.
„ 21	96	1022	14·67	1408	
„ 22	94	1030	16·35	1536	
„ 23	124	1020	9 45	1171	
„ 24	104	1023	10·89	1132	
„ 25	104	1022	10·89	1132	
„ 26	120	1022	9·72	1166	
„ 27	96	1021	9·	864	
„ 28	79	1030	13·32	1052	
„ 29	93	1033	18·45	1715	Animal diet with 8 oz. of gluten bread.
„ 30	86	1035	20·55	1767	
„ 31	66	1038	19·98	1318	Animal diet, with 4 oz. of gluten bread and 4 oz. of almond biscuit.
Feb. 1	81	1036	19·98	1618	Animal diet, with 6 oz. of gluten bread and 2 oz. of almond biscuit.
„ 2	78	1035	15 63	1219	Animal diet, with 8 oz. of almond biscuit.
„ 3	73	1034	18·45	1346	
„ 4	72	1033	17·13	1233	
„ 5	56	1031	12·	672	
„ 6	Urine not examined				Animal diet, during the first two days, then animal diet with gluten bread.
„ 7					
„ 8					
„ 9					
„ 10					
„ 11					
„ 12	108	1033	19·44	2099	Animal diet, with 8 oz. of gluten bread.
„ 13	104	1033	17·55	1825	
„ 14	109	1032	15 63	1703	
„ 15	78	1033	16·74	1305	Animal diet, with 8 oz. of almond biscuit.
„ 16	88	1033	15 63	1375	
„ 17	83	1027	10·74	784	
„ 18	80	1031	14·40	1152	
„ 19	74	1032	15·	1110	

Wm. N—, æt. 44. Diabetes of three months' standing. No complication.

Date.	Quantity of urine.	Sp. gr.	Proportion of sugar to fluid-ounce.	Quantity of sugar per 24 hours.	Diet.
	oz.		grains.	grains.	
April 11	180	1040	40·	7200	Full mixed diet.
„ 12	190	1036	40·	7600	
„ 13	180	1036	37·50	6750	
„ 14	160	1041	40·	6400	
„ 15	220	1035	38 70	8514	
„ 16	230	1038	41 35	9510	
„ 17	190	1037	40·	7600	
„ 18	60	1042	38·70	2322	Animal diet, with greens and 8 oz. of almond bread.
„ 19	80	1017	8·20	656	
„ 20	70	1019	9·	630	
„ 21	70	1030	18 45	*1291	Animal diet, with greens.
„ 22	95	1029	21·81	*2071	

In carrying out the principle of avoiding the ingestion of starch and sugar in Diabetes; it happens, on account of the extensive diffusion of these materials through the vegetable kingdom, that, not only must bread and other kinds of corn food, whether derived from barley, oats, rye, maize or rice, be prohibited; but, likewise, with very slight exception, all the vegetable alimentary substances in common use, as will be seen from the following examination:

The leguminous seeds, including peas, garden or broad beans and the French bean or haricot, like the cerealia, contain a large per-centage of starch.

Macaroni, vermicelli, Italian paste and semolina are prepared from wheat, and, as may be shown by the tincture of iodine, abound in starch.

The farinaceous foods such as sago, tapioca and arrow-root must of course be avoided.

* There happened to be no more almond bread to continue with, so the patient was ordered an animal diet with greens only; but it is probable he did not confine himself strictly to what was allowed. He was seen toasting some bread one of the two days referred to.

In the potato, the starch varies, according to the analyses referred to in Dr. Pereira's work on 'Food and Diet,' from 9·1 to 24·4 per cent.

Carrots, parsnips, beetroot, turnips and radishes contain sugar.

As regards the other kind of vegetables, it may be said, that anything white contains sugar; so that cauliflower, brocoli, cabbage, seakale, celery, and asparagus prove objectionable. When, however, through the influence of the sun's rays upon the growing vegetation, green colouring matter has become developed, the saccharine matter will be found, at the same time, to have disappeared. Thus, the green tops of asparagus are devoid of sugar whilst the white part below is freely charged with it. Such is, also, what may be noticed with the green leaf and the white stalk of the celery plant. Indeed, anything in the way of leaf and leaf stalk that has been allowed to become green by exposure to light may be considered unobjectionable in Diabetes; which will therefore permit of such articles as greens and spinach being freely consumed as vegetables.

Amongst salads, the patient may be guided by the remark that has just been made. Water-cress are quite free from sugar; and, therefore, being so readily procurable all the year round, form an important article of consideration as a relish for the Diabetic. Lettuce that has been allowed to become green may be taken, but that which is without colour should be rejected. Radishes and celery, although they contain sugar, yet they do not contain it in such quantity, as altogether to prohibit their use sparingly and occasionally. They are free from starch.

All fruits, on account of their saccharine properties, must be scrupulously avoided. For dessert, therefore, the Diabetic has to fare very badly. The oily seeds, such as the hazel-nut, filbert, walnut, brazil-nut, sapucaia-nut and

almond might be partaken moderately of; but, it must be remembered, they are not entirely devoid of sugar, although there is a total absence of starch.

As a part of the dietetic treatment, attention must be given to what is drunk as well to what is eaten. Milk is a fluid which is often recommended to be consumed in quantity, on account of its belonging to an animal regimen. It should be taken into consideration, however, that amongst the constituents of milk there is a saccharine principle—lactin, which is present in cows' milk, according to various analyses, to the extent of 3 or 4 to 6 per cent. It would be an unintelligible exception if the presence of lactin did not render milk a prejudicial article for consumption in Diabetes. But, that it does produce a prejudicial effect upon the urine, may be seen on reference to North's case. During the five days, February 16th—20th, that this patient was taking milk—three pints a day—the elimination of sugar steadily increased from 1198 to 2225 grains for the twenty-four hours; and, on the following day, fell to 927 grains, the milk having been taken off and two pints of beef tea substituted.

The least saccharine wines, and brandy, constitute the most suitable beverages, belonging to the fermented class, to be given in Diabetes. But, looking to the great variation that is known to exist in the amount of sugar contained in different wines, and different specimens of the same kind of wine; which cannot be always correctly judged of by the taste: at my request, Mr. Kendray, of 19, Great Queen Street, Lincoln's-Inn-Fields, supplied me with a number of samples for analysis, the particulars of which, I may mention, I have furnished him with, for the sake of those to whom such information might prove useful. Sherries are infinitely preferable to port on account of their much greater freedom from sugar. One of Mr. Kendray's

samples of sherry—a vino de Pasto—I found entirely free from sugar; another, an ordinary sherry contained only traces; whilst the others possessed saccharine properties in varying degrees of extent. A specimen of old port contained rather over 17 grains of sugar to the fluid ounce. The port wine now in consumption at Guy's Hospital contains $37\frac{1}{2}$ grains of sugar to the ounce, and the sherry 16 grains. Of three clarets, of different qualities, that were sent to me, neither contained more than traces of sugar. A specimen of pale brandy yielded me one grain of sugar to the ounce. The mode of colouring the French brandy will account for this sugar; the spirit as it comes from the still being, of course, not only colourless but absolutely free from saccharine principle.

Sweet wines, sweet ales, porter and stout should be rigidly forbidden; but the Burton bitter ale is not so saccharine as altogether to preclude it from occasional and moderate use. On testing a specimen of Bass's bottled bitter ale, which, I had sent for, in the ordinary way, to a common retail house, it only yielded me just over 2 grains of sugar to the ounce; the draught bitter ale, Bass's and Allsop's, afforded me rather more; whilst a specimen of mild ale gave me nearly 13 grains to the ounce.

Amongst non-stimulating beverages, either tea, coffee, or cocoa from the nibs, may be taken. Soda-water proves exceedingly grateful to the patient's feelings, and may be freely allowed. Lemonade, on account of the sugar it contains, must be avoided.

Founded on the principle that has been advocated, a dietary scheme has been framed, giving in a tabular view what kind of food and drink may be taken, and what is to be avoided. The following dietary for the Diabetic includes reference to the chief articles in ordinary consumption amongst us:

DIETARY FOR THE DIABETIC.

MAY EAT

Butcher's meat of all kinds, except liver.
Ham, bacon, or other smoked, salted, dried, or cured meats.
Poultry.
Game.
Fish of all kinds, fresh, salted, and cured.
Animal soups not thickened, beef-tea, and broths.
The almond, bran, or gluten substitute for ordinary bread.
Eggs dressed in any way.
Cheese. Cream cheese.
Butter. Cream.
Greens. Spinach.
Water-cress. Mustard and cress. Green lettuce.
Celery and radishes may be partaken sparingly of.
Jelly, flavoured, but not sweetened.
Blanc-mange made with cream, and not milk.
Custard made without sugar.
Nuts of any description, sparingly.

MUST AVOID EATING

Sugar in any form.
Bread, wheaten or otherwise.
Rice. Arrowroot. Sago. Tapioca. Macaroni. Vermicelli.
Potatoes. Carrots. Parsnips. Turnips.
Peas. French beans.
Cabbage. Brussels sprouts.
Cauliflower. Broccoli.
Asparagus. Seakale.
Pastry and puddings of all kinds.
Fruit of all kinds, fresh and preserved.

MAY DRINK

Tea. Coffee. Cocoa from nibs.
Dry sherry. Claret.
Brandy, and spirits that have not been sweetened.
Soda-water.
Burton bitter ale, sparingly.

MUST AVOID DRINKING

Milk, except sparingly.
Sweet ales, mild and old. Porter and stout.
All sweet wines. Port wine, unless sparingly.
Liqueurs.

Directly antagonistic in principle to the mode of treatment that has been advocated, are two plans that appear to have originated with M. Piorry, of Paris, one of which may be called the dry, the other the homœopathic plan —homœopathic, I mean, not in respect of minimum doses, but in its literal sense of *similia similibus curantur.*

From what I have seen regarding the disease, I cannot help thinking that abstinence from drinks must be exceedingly calculated to lead to prejudicial consequences; and it is known to produce the utmost discomfort. The sensation caused by privation from fluids in Diabetes must be something truly horrible to bear; and, I believe, few are to be found who would submit to endure it for any length of time. I hold that a supply of fluid— such as will not lead to the augmentation of sugar from its own constitution— is beneficial in Diabetes, as it enables the sugar to be carried away; thus, leaving the blood less highly concentrated, and therefore presenting a less extent of deviation from the state that is normal. I believe that good is done by allowing the patient to quench his thirst, as long as he does so with an appropriate fluid. It is nothing but obeying the dictates of nature, that fluid should be taken when its requirement is indicated through the sensation of thirst.

I cannot certainly understand, how any case of Diabetes mellitus can have been benefited by the administration of sugar; and yet, cases have been placed on record where such a result is described. My own experience has always tended most strongly to the conclusion, that the ingestion of sugar cannot be otherwise than followed by an aggravation of the symptoms. I once saw a patient, under one of my colleagues, treated in Guy's Hospital by the sugar method; but the poor fellow was rendered so much worse, that it was obliged to be discontinued after a couple or three days.

There is a paper by Dr. Sloane in the 'British Medical

Journal,' May 29th, 1858, on the saccharine treatment in Diabetes mellitus. The plan was tried upon three patients in the Leicester Infirmary. In the first, half a pound of treacle a day was given and continued for more than nine weeks. The urine was increased by three quarts a day, or more than doubled, and the sugar rose from fifty-six to eighty grains per fluid-ounce. Upon an opposite plan of treatment being resorted to, the quantity of urine, and proportion of sugar it contained, soon underwent a very marked decline. In the second, sugar was administered for a month; half a pound of treacle during the first three days, and then, the same quantity of honey during the remainder of the time, as the treacle induced nausea, and made the patient feel very thirsty. At the end of the month there was much greater weakness, a loss of two and a half pounds in weight, and much more thirst. The honey was, therefore, ordered to be omitted. The quantity of urine had increased from eight to fourteen pints, but it only contained one grain extra of sugar per ounce. In the third case, half a pound of treacle was given daily from November 28th, 1857, to January 16th, 1858, to a girl fourteen years of age, who was labouring under the complaint. The patient lost four pounds in weight. The urine increased in amount to the extent of one pint for the twenty-four hours, and the proportion of sugar per fluid-ounce rose from sixty-four to seventy grains. "The "girl," says the report, "was discharged at her own request, "and died on February 17th, at her home in the country." Strangely enough, notwithstanding these results, which form the basis of his paper, Dr. Sloane concludes, by stating that much has yet to be learned concerning the mode of using sugar in glucosuria, and submitting suggestions (as if it had proved a desirable remedy) on the kind of sugar to be used and the mode of using it.

I read in the 'Lancet,' January, 1862, that M. Rigodin

allows a diet in which sugar-yielding substances play a great part; because, the experiments of Dumas and Bernard, having proved that sugar in the economy is indispensable to life, he thinks it is rational to *give* sugar in Diabetes, as patients suffering from this malady lose large quantities of it. By depriving these patients, "continues the paragraph," of fecula sugar, they must be made worse, for they are deprived of the means of making up for saccharine losses. To act upon such a line of argument as is here adduced would be, to adopt a course, which general experience condemns as in the highest degree pernicious to the patient, under a process of reasoning as unfounded as it is extravagant.

Upon the medical treatment of Diabetes I shall make but very few remarks. No medicinal agent, as far as I am aware, has yet been found that possesses the power of permanently diminishing (I introduce the word permanently on account of the remarks I shall have to make with reference to opium) the elimination of sugar in Diabetes. All that can be at present expected from medical means is to fortify the strength of the patient, to remove whatever collateral functional error may exist, and, to alleviate symptoms depending on structural disorganization endeavouring at the same time to check its advance. An alkaline plan of treatment is that, perhaps, which has received most favour. The Vichy water has been strongly recommended on the Continent, and is pretty largely used in this country. Cases not unfrequently occur, where, however, the mineral acids are indicated; and, administered with a bitter, give rise to a desirable result, by improving the tone of health of the patient. Indeed, the therapeutic management of our case must be conducted upon the ordinary principles belonging to our art; the object to be kept in view, being, to resort to the use of such mea-

sures as are best calculated to improve the tone of system of the Diabetic.

It is a matter of common observation in hospital practice to notice that, no matter what the remedial agent employed, the condition of the patient for a time improves after reception into the hospital. I expect the regular and salutary hours, the regular living, and the wholesome food (I am not speaking now of cases restricted to an animal regimen), have much to do with producing this effect. Some few months ago, a patient was sent to me from the country as a Diabetic. On admission into the hospital, his urine was found to be natural in quantity, but to contain a small amount of sugar. He was not placed upon a restricted diet, but, in the course of a few days, the urine was totally devoid of any saccharine character.

Although there is no remedy known which possesses a direct curative influence over Diabetes, yet patients do recover from this complaint. In one case I had an opportunity of noticing a gradual decline of the sugar until it permanently disappeared; and this, in a form of the complaint, that had originally been very severe. The patient had, first of all, been an inmate of the hospital under one of my colleagues for a few months. He afterwards attended as an out-patient, and continued under my care for very many weeks. His urine was tested occasionally; and, for a long time, found always to be highly charged with sugar. After a while, however, to my astonishment, on one day applying the test, I met with a much less reaction; and, soon, there was no reaction at all. The patient continued still to attend for a few weeks longer, but the urine retained a natural character. This case occurred about four years ago, and I have not heard of the patient since. There was no restriction of diet resorted to whilst he was under my care, and the medical treatment consisted principally

of a carbonated alkali, with the carbonate of ammonia and opium.

A striking feature belonging to Diabetes is the tolerance of opium by persons labouring under the complaint. I have not unfrequently commenced with two and three grains of opium a day, without producing any visible effect. M'Gregor* has placed upon record two cases where he gave opium in very large doses. In one, the quantity was gradually increased, till it reached a drachm in the twenty-four hours. The urine was diminished from 18 lbs. to 2 lbs. and 3 lbs. in quantity. When, however, the opium was discontinued, the thirst increased, and along with it the quantity of urine. In the other case, the administration of opium was continued for nearly three months, and the dose was steadily increased, till it amounted to ninety grains daily. The symptoms of the patient were ameliorated, and the urine was, as in the first case, most materially reduced in quantity. The opium was gradually diminished to a few grains daily; and, after this, the patient failed rapidly, the Diabetic symptoms increased, and a pleuritic attack subsequently produced a fatal termination. In each case, during the administration of the opium, the urine became very highly charged with urea; and, was likewise at one time alkaline from the presence of carbonate of ammonia.

Another feature of peculiarity belonging to the complaint, is the inability that is experienced to render the urine alkaline by the administration of the fixed alkalies and their vegetable salts. Although I have given the carbonate of soda to the extent of four drachms a day; the acetate of potash, half an ounce; the tartrate of potash and soda or Rochelle salt, six drachms, and even an ounce; and the citrate of potash, six drachms; yet, I have never succeeded

* 'London Med. Gaz.,' May, 1837.

in rendering the urine alkaline, or in any way approaching this character. It is only in one patient that I have ever seen the urine alkaline in Diabetes. This was in Thair's case (Case IV, Appendix) after his admission into the hospital the second time. He was in a very feeble condition, and his urine was repeatedly, during a period of about three weeks strongly alkaline to test-paper, and threw down a copious deposit of phosphates. The alkalinity, however, was due to the presence of ammonia, and the patient at the time was taking nothing to account for the phenomenon.

I do not consider it necessary to refer to the various remedies that have been proposed, from time to time, upon theoretical grounds for Diabetes as one idea or another has prevailed concerning its nature. The theories themselves have proved, by the progress of knowledge, to be illusory; and, it is not likely, therefore, that what has been founded upon them, could really produce any benefit. I have never seen anything recorded, to give me an atom of faith in anything that has been thus proposed.

The complications that arise during the existence of the Diabetic condition must be treated upon ordinary principles. But, I feel strongly convinced, that a most powerful aid towards attaining the object that is desired, is to be obtained, by adopting the dietetic measures that are known to reduce the intensity of the Diabetic state, at the same time that remedial measures are directed towards the special disorder that exists. The greater the approximation to a healthy state of the fluids that can be produced, the greater will be our chance of success in overcoming whatever secondary disorder may exist for us to treat.

APPENDIX OF CASES.

CASE I.

The following case contains the results of a close examination of the influence of various articles of food upon the elimination of sugar in Diabetes. The early history of the patient, which I have extracted from the ward report-book, runs thus:

Joseph North, æt. 32, No. 34, Stephen Ward, Guy's Hospital. A light-haired, fresh-complexioned man, who, previous to his present complaint, had scarcely known a day's illness to have happened to him. During the commencement of November, 1860, his disease set in; according to his own account, under the following circumstances: He had been drinking to excess, and was suffering from retention of urine, for the relief of which he procured an ounce of the sweet spirit of nitre, and took it in two doses within the space of five hours. He drank copiously afterwards of linseed tea, and in a few days' time noticed that he began to pass an unusual quantity of urine, which continued increasing in amount, until, within a week, it reached, he said, as much as six to seven quarts during the day, and from three to four during the night. He now suffered from much pain in the loins, and found himself rapidly wasting, with a voracious appetite, an insatiable thirst, and a dry skin.

The patient presented the appearance of being a strong-

constitutioned man, although he carried with him the characteristic diabetic aspect. He complained of nothing but what was attributable to his special disorder, and there was no evidence of his lungs or other organs being otherwise than perfectly sound; so that he formed an instance of a pure, uncomplicated case of Diabetes at a middle period of life. He also possessed an amount of intelligence surpassing what is usually met with in persons of his sphere of life, and with readiness and willingness consented to implicitly carry out whatever instructions might be given him. Time showed that he entered enthusiastically into the spirit of the observations that were being conducted. I was dependent upon him, not only for obedience to my instructions and for giving an exact account of all that he partook, but also for attention to the collection of his urine in separate vessels for the several periods into which the twenty-four hours were designedly divided. He was under the surveillance of the attendants and the other patients in the ward, of whom I made inquiries to see if his statements could be relied upon. From the information I obtained, and from the consistency in the results of my examination of his urine, I have the strongest reason to believe that his share in the inquiry may be taken as performed with as much exactitude as is well possible to have been observed. In every respect, indeed, I regard him as one of the most desirable persons I could have alighted upon for carrying out the investigation that was conducted.

It may be desirable to state that every analysis given in the case was made by myself; but, there are two students to whom my thanks are specially due for their assiduous attention in connection with it. To Mr. Cookson I am indebted for assistance rendered in the ward, and to Mr. Lamb for assistance rendered in the laboratory. Without exception, the analyses were made from day to day, as soon as the twenty-four hours had expired, so as to avoid loss of sugar from decomposition. The determination of the quantity of sugar was effected by the cupro-potassic liquid in the manner I have elsewhere described.

The patient had been an inmate of the hospital for six weeks before he fell under my care. He was admitted under my

colleague, Dr. Gull, December 20th, 1860. I perceive he was then passing from eleven to fourteen pints of urine a day. He had been placed upon full diet, with milk, greens, and four ounces of brandy and two bottles of soda-water daily. His medicine at first consisted of the sesquicarbonate of soda in scruple doses, with the compound tincture of cinchona, out of infusion of hop three times a day, and half a grain of opium also three times a day. This was afterwards changed for the decoction and compound tincture of cinchona, with one grain of opium three times a day. Under this treatment, with the diet already mentioned, he continued until February 1st, when he was transferred to my care, on account of Dr. Gull then taking charge of the clinical wards.

The object I had in view, was to examine as carefully as possible the influence exerted by the common articles of diet over the extent of elimination of sugar—to ascertain, in fact, as closely as I could, the relation existing between the food ingested and the sugar voided by the patient. In order to get the separate effect of his meals, I divided the twenty-four hours into periods of four hours each, and directed him to collect his urine in separate vessels for each. He was always to empty his bladder when the end of the period had arrived, and to be careful that no loss of urine occurred when he had an alvine evacuation. I had thus six specimens of urine for examination every day; and attention was directed to the quantity, specific gravity, extent of saccharine impregnation, and the general properties of each visible to the eye.

A synopsis of all the results obtained will be found arranged in a tabular form further on, with an account of the diet that was from day to day taken. On reference to this table of results, there will be observed a series of columns, the first of which is occupied by the date; next is a column for the several periods into which the day was divided; and in a line with each particular period, in succeeding columns, can be seen the specific gravity of the urine belonging to it, the quantity of urine passed, the number of grains of sugar in the urine per ounce, and the gross amount of sugar for the four hours, which has been, of course, obtained by multiplying the quantity of sugar per ounce with the quantity of urine passed. In the

next column is the total amount of sugar for each twenty-four hours. Then comes the diet, with the hour mentioned at which the various articles of food were taken. Lastly, is a column for remarks as to the state of the urine, and other prominent particulars.

The amount of sugar passed from day to day, and the account of the food consumed, have furnished materials from which has been framed a diagramatic plan, showing at one view the relation existing between the ingestion of different articles of diet and the elimination of sugar in Diabetes. This plan has been, for the sake of convenience, introduced into a previous part of the volume (*vide* p. 140). The fluctuating line points opposite each day (and under this the diet taken is mentioned) to the number of grains of sugar passed. The horizontal lines corresponding to the quantities indicated by the scale of grains placed at either side; on looking to the position of the fluctuating line for any particular day, the amount of sugar passed may be read off within a moderate amount of minuteness.

In the daily report that follows, I have only selected from the details of the case what I considered to be the leading features of interest. To have done more would have given an unnecessary prolixity to the case. The whole of the results observed being systemically arranged together in a tabular form further on, reference can always be made to it, for details that may not happen to be found in the report. It will be noticed, I have not hesitated, freely to introduce comments upon any particulars that I have thought worthy.

Throughout the observations on the effect of diet that were conducted, the patient took the one grain of opium three times a day that had been formerly prescribed for him; but all other medicine was omitted. He also continued his brandy and soda-water, four ounces of the former and two bottles of the latter *per diem*, and was allowed tea without milk or sugar in it, and water *ad libitum*. For the first few days, it will be observed, that the diet given was an ordinary full mixed one. It was the same that he had been taking ever since his admission into the hospital; and he was purposely ordered thus to continue with it for a short time, so that a fair starting-point

might be obtained to rightly judge of what effects might be produced.

February 1st and 2nd.—These twenty-four hours extended from one o'clock the middle of one day to one o'clock the middle of the next. The diet was a full mixed one. The amount of sugar passed was 9475 grs.; the amount of urine 233 oz. The six specimens gave as their mean sp. gr. 1042, and as the mean quantity of sugar per oz. 40·97 grs. On looking at the six specimens placed side by side, it was striking to observe how much darker in colour that passed between one and five in the morning was than either of the rest.

5th.—The twenty four hours' period was now made to commence from one o'clock in the morning instead of the afternoon, in order to get the effect of each day's diet manifested upon the urine belonging to that day. The reason for passing from the 2nd to the 5th was, that the urine for two days escaped being examined, the system of procedure not having been then definitely marked out.

To-day the patient was upon a full mixed diet. Quantity of sugar 10,573 grs.; quantity of urine 259 ozs.; mean sp. gr. of the six specimens of urine 1037; mean amount of sugar per oz. 41·29 grs. The quantity of sugar passed between 1 and 5 a.m. was 593 grs., and between 5 and 9 a.m. 889 grs.; whilst during the remainder of the day it was upwards of 2000 grs. for each four hours. There is thus a striking increase in the extent of production of sugar occasioned by the immediate influence of the meals. The urine passed between 1 and 5 a.m. was of nearly a healthy yellow colour, the other specimens were pale and diabetic looking.

6th.—Full mixed diet again. Quantity of sugar 8961 grs.; quantity of urine, 205¾ oz.; mean sp. gr. 1039; mean amount of sugar per oz., 44·05 grs. As upon the preceding occasions, the urine passed between 1 and 5 a.m. was of a much darker colour than any of the rest.

7th.—To-day the diet was changed, and the patient allowed only to take animal food. He had two mutton chops, 3 oz. of dressed meat, and two pints of beef tea. The average weight of the mutton chops supplied in the hospital may be taken from observation at about 7 oz. The quantity of

sugar immediately fell to 2474 grains; quantity of urine only $77\frac{1}{4}$ oz. The specific gravity for the first two periods of the day was 1043 and 1046; for the last it had descended to 1025. Again, during the first two periods of the day the urine was charged with sugar to the extent of 44·80 and 41·30 grs. per oz.; during the last there was only 20·15 grs. per oz. All the urine now presented a fair amount of colour, and that passed in the afternoon between 1 and 5 threw down a lithate deposit.

8th.—The diet consisted of three chops and three pints of beef tea. Quantity of sugar 3035 grains; quantity of urine 143 oz. The urine passed between 5 and 9 in the morning was of the highest sp. gr., viz. 1037; that passed between 9 and 1 at night, the lowest, viz. 1013. A corresponding order was noticeable in the degree of saturation with sugar. During the former of the two periods the urine contained 26·35 grains per oz.; during the latter 13·45.

All the specimens of urine were again pretty fairly coloured, and that passed during the afternoon threw down a deposit of lozenge-shaped crystals of lithic acid.

The patient expressed himself as feeling much less thirsty, and said that during the previous night he had been in a good perspiration, such as he had not been in before for the last three months.

9th.—Two chops, 4 oz. of dressed meat, 1 egg, and 3 pints of beef tea for diet. Quantity of sugar 2471 grs.; quantity of urine $127\frac{1}{4}$ oz. There was no sp. gr. observed exceeding 1030. The specimen presenting the lowest was again that from 9 to 1 at night. It was 1013, and contained 12·10 grains of sugar to the oz. In the other specimens the sugar per oz. did not exceed 24·45 grs.

10th.—The diet allowed was 1 chop, 1 lb. of dressed meat, 1 egg, and 3 pints of beef tea; but the patient took also, at 4 p.m., a pint of prepared cocoa sweetened with sugar. It was Sunday afternoon, and his friends had been to see him and brought him this as a treat. Henceforth, he was stringently directed not to have anything brought in, and watch was put upon him to see that it was carried out. Quantity of sugar passed 3179 grs,; quantity of urine 136 oz. It is ex-

ceedingly interesting to look at the amount of sugar passed during the different periods of the day, and notice the immediate effect produced by the violation of his instructions that he committed. From 1 to 5 a.m. it was 290 grs.; 5 to 9 a.m. 166 grs.; 9 to 1 p.m. 461 grs.; 1 to 5 p.m. 468 grs.; 5 *to* 9 *p.m.* 1311 *grs.*, and 9 to 1 a.m. 483 grs. He partook of the sweetened cocoa at 4 p.m.

Between 1 and 5 p.m. the sugar was as nearly as possible the same as during the preceding four hours. But between 5 and 9 p.m. he passed upwards of 800 grs. more than he might have fairly been expected to have done, had he not departed from his prescribed regimen. During the following four hours the amount fell to nearly what it was before the rise. It thus seems that what was taken at 4 p.m. manifested its principal effects between 5 and 9 p.m., and that the sugar the result of ingestion was all carried off, or had, so to speak, filtered through the system and been discharged, by the end of this time. The urine, which had been previously more or less high coloured, became during the last two periods of the day almost as colourless as water.

11th.—One chop, 13 oz. of dressed meat, and 3 pints of beef tea for diet. Quantity of sugar passed 3329 grs.; quantity of urine 140¼ oz. It appeared as if his infraction of the previous day had still left traces of its ill effects for several hours. The sugar continued high in quantity up to 5 p.m. to-day. Between 1 and 5 p.m., in fact, he passed 1016 grs.; the urine having a sp. gr. of 1035, and containing 33·30 grs. of sugar to the oz. From 5 to 9 p.m., the amount of sugar was 418 grs.; and during the next four hours only 200 grs. This last urine was of a sp. gr. 1013, and contained 7·40 grs. of sugar to the oz.

12th.—Diet, 1 pair small soles, 1 chop, 4 oz. of dressed meat, and 3 pints of beef tea. Quantity of sugar 1096 grs.; quantity of urine 92¾ oz. The last specimen of urine—that passed between 9 and 1 at night, was again of a sp. gr. 1013, and contained only 6 grs. of sugar to the oz. The highest degree of saturation with sugar attained to-day was 19·35 grs. to the oz. Some of the urine again deposited lithates and lithic acid.

13th.—The diet consisted of fish and beef tea only. Four pairs of small soles and 2 pints of beef tea were consumed. Quantity of sugar 1696 grs.; quantity of urine 163 oz. The quantity of sugar per oz. did not reach higher than 15·15 grs., and for the last period of the twenty-four hours descended to 5·83 grs. Three of the specimens of urine threw down crystals of lithic acid.

14th.—Diet, 2 pints of jelly, 16 oz. of dressed meat, and 1 pint of beef tea. The jelly was made in the usual way, and flavoured with sugar, a little sherry, and essence of lemon. Quantity of sugar passed 952 grs.; quantity of urine 107½ oz. The sugar in the jelly, of which I was informed 2 oz. were allowed for the 2 pints, evidently exerted a deleterious tendency. The jelly was all taken at 6 and 9 in the morning, and no other food till 1 p.m. Between 9 and 1 in the day, the urine presented a sp. gr. of 1031, and contained 25·25 grs. of sugar to the oz. During the other portions of the day, the amount of sugar per oz. was not higher than 13·15 grs., and during the last two periods was only 3·33 and 3·69 grs. respectively.

15th.—Diet, two pints of jelly, two chops, and two pints of beef tea. Quantity of sugar passed, 569 grs.; quantity of urine, 107¾ oz. The jelly to-day was made without sugar. One pint of it was taken at 6 a.m., and the other at 10 a.m., with no other food till 1 p.m. The sugar passed was infinitely lower in quantity than it had ever been noticed before. The proportion per ounce of urine did not exceed 7·56 grs., and the lowest was 2·35. The sugar voided during the respective periods of the twenty-four hours was 76, 30, 56, 86, 67, and 254 grs. Looking at these figures, it would appear that jelly does not occasion so much production of sugar as meat. Up to 1 p.m. nothing but the jelly was taken; and after 1 p.m. the meat and beef tea. The amount of sugar continued exceedingly low up to 9 p.m., but during the succeeding four hours the quantity was 254 grs.—nearly half of the amount given for the twenty-four hours. I presume this may be taken as the effect of the change to meat, and that the sugar resulting from the ingestion of meat requires a longer time to make its appearance in the urine than that resulting from the

ingestion of saccharine and starchy materials. The patient confidentially stated to-day to the reporter of his case, that he felt certain he was getting better on account of a particular symptom he had observed. To use his own words, "He had been like a child hitherto during his illness, but now he found that the nature of man had returned upon him." His weight to-day was 9 st. 5 lbs.

16th.—Diet, one chop, sixteen ounces of dressed meat, and three pints of milk instead of the beef tea. Quantity of sugar, 1198 grs.; quantity of urine, 105½ oz. The lowest amount of sugar per ounce noticeable to-day was 7·65, which is a trifle higher than the highest of yesterday—a fact that presents a significant appearance with respect to the substitution of milk for beef tea.

17th.—Diet precisely the same as yesterday. The amount of sugar passed was 1258 grs.; quantity of urine, 98¼ oz. Mean sp. gr. of the six specimens 1026. Mean amount of sugar per oz. 14·96 grs. The patient stated that a much less amount of food satisfied him now than formerly, and that he did not experience thirst as he used to do.

18th.—Diet the same as yesterday, with the addition of half a pound of suet, which was mixed with the milk. Quantity of sugar, 1485 grs.; quantity of urine, 99½ oz. Mean sp. gr. given by the six specimens, 1025. Mean amount of sugar per ounce, 16·95 grs.

19th.—Diet, two chops, eight ounces of dressed meat, three pints of milk, and three quarters of a pound of suet. Quantity of sugar, 1722 grs.; quantity of urine, 83¾ oz. Mean sp. gr. of the six specimens, 1031. Mean amount of sugar per ounce, 21·62 grs.

20th.—Diet, one chop, sixteen ounces of dressed meat, three pints of milk, and a quarter of a pound of suet. Quantity of sugar, 2225 grs.; quantity of urine, 101½ oz. Mean sp. gr. of the six specimens, 1033. Mean amount of sugar per ounce, 23·47 grs.

According to the patient's opinion—and he had not been informed of the analytical results obtained—the milk he was having did not agree with his complaint; for since he had

been taking it, he began to experience somewhat similar sensations to those he had at the commencement of his disease. He liked the suet, and said it filled up the void which seemed to exist at the pit of his stomach, more than anything else he had yet taken. In strict accordance with the statement that was expressed by the patient, the condition of his urine had been steadily getting worse, during the whole period (five days) that he had been supplied with milk. It is true, during the latter three days he had also been supplied with suet; but the effect observed would not appear to be due to this; for, the last day, when there was a rise of 500 grs. of sugar, he only had a quarter of a pound of suet; whilst on the day previously he took three quarters of a pound, and the day before that half a pound.

21st.—Diet, six eggs, one pound of dressed meat, and two pints of beef tea. The quantity of sugar descended to 927 grs., and the amount of urine to 65 oz. Three of the specimens of urine again deposited crystals of lithic acid.

The patient's weight to-day was 9 st. 5 lbs., being exactly the same as when he weighed on the 15th.

22nd.—Diet, eight eggs, two chops, and two pints of beef tea. The quantity of sugar again fell, amounting only to 734 grs. The quantity of the urine was $71\frac{3}{4}$ oz. Some of the urine threw down lithates and crystals of lithic acid.

23rd.—Diet, four dozen oysters, one pound of dressed meat, and three pints of beef tea. Quantity of sugar, 1310 grs.; quantity of urine, $115\frac{3}{4}$ oz.

24th.—Diet precisely the same again as yesterday; but the quantity of sugar was only 896 grs., and the quantity of urine 97 oz. Looking to these two days' results, the effect of oysters upon the production of sugar in the complaint is not very clear. I had expected to find the amount of sugar increased on account of the considerable quantity of amyloid substance which the oyster contains, and this expectation was realised by the first day's examination; but on the second day the sugar fell to quite a moderate degree. Between 1 and 5 in the afternoon, the four hours' urine only contained 16 grains, and was charged merely to the extent of 2·4 grains to the ounce. The oysters had been all taken at 7 o'clock in the

morning, and throughout the day until night the sugar continued low.

25th.—Diet, one chop, eight ounces of dressed meat, and three pints of beef tea. Quantity of sugar, 867 grs.; quantity of urine, $78\frac{1}{4}$ oz.

26th.—Diet, eight ounces of suet, three chops, and three pints of beef tea. Quantity of sugar, 886 grs.; quantity of urine, 103 oz.

Between 1 and 5 p.m. the urine that was passed only contained a trace of sugar. This was the first time that I had noticed any specimen in such a state.

The suet was given with only meat and beef tea, to clear up the point that was left unsettled when it had been given with milk. The result obtained clearly showed, as had been surmised, that the rise before observed was not due to the suet.

27th.—Diet, four ounces of gum arabic made into a dilute mucilage, and drunk at intervals during the morning, one chop, one pound of dressed meat, and two pints of beef tea. The quantity of sugar ascended from 886 to 1177 grs.; the amount of urine was 95 oz.

The patient weighed to-day 9 st. 10 lbs., being an increase of 5 lbs since the 21st.

28th.—Diet, one chop, one pound of dressed meat, and three pints of beef tea;—the same diet as yesterday, substituting one pint of beef tea for the four ounces of gum. The quantity of sugar descended, returning to 887 grs. The amount of urine was $71\frac{1}{2}$ oz. The specimen passed between 1 and 5 p.m. threw down lithates.

March 1st.—Diet, three chops, greens, and two pints of beef tea. Quantity of sugar, 594 grs.; quantity of urine $65\frac{1}{2}$ oz.

For the second time, one of the specimens of urine contained a trace of sugar only; and, as on the previous occasion, it was the urine passed between 1 and 5 in the afternoon. Once before, there was a particularly small quantity of sugar (sixteen grains) eliminated for the four hours, viz., on February 24th; and this also occurred during the period from 1 to 5 p.m.

Four of the specimens to-day deposited lithates.

2nd.—Diet, seventeen ounces and a half of bran biscuits, one chop, one pound of dressed meat, and three pints of beef tea. The quantity of sugar rose to 2630 grs.; amount of urine, $96\frac{1}{2}$ oz. The bran biscuits were commenced at 6 a.m.; and even the urine passed between 5 and 9 a.m. bore evidence of their effects, for the amount of sugar per ounce sprang from 15·99 grs. to 29·37, and continued higher than this till 9 p.m., when it made a slight descent to 26·64 grs.

The patient soon perceived in his bodily sensations the prejudicial effect of his alteration of diet. About the middle of the day he said he did not know how the bran biscuits would suit him, but he thought they made him thirsty, and his mouth dry.

3rd.—Diet, twelve ounces of bran biscuits, one chop, one pound of dressed meat, and three pints of beef tea. Quantity of sugar passed, 3291 grs.; quantity of urine, $110\frac{1}{4}$ oz. The proportionate amount of sugar in the urine fluctuated between 22·50 and 36 grs. per ounce, and the sp. gr. between 1030 and 1043.

The patient felt quite sure the bran biscuits were not agreeing with him; for not only was he thirsty, and his mouth dry, but the skin of his hands, instead of being supple as it had lately been, was dry and harsh.

4th.—Diet, twelve ounces of gluten bread, one chop, twelve ounces of dressed meat, and three pints of beef tea. Quantity of sugar passed, 4079 grs.; quantity of urine, 151 oz. The proportion of sugar in the urine fluctuated between 23·58 and 30·63 grs. per ounce, and the sp. gr. between 1031 and 1038.

5th.—Diet, five ounces and a half of gluten bread, three chops, four ounces of dressed meat, and three pints of beef tea. It will be observed that five ounces and a half of gluten bread were taken to-day, instead of twelve as yesterday. The quantity of sugar fell to 3228 grs., and the quantity of urine to $121\frac{1}{2}$ oz. The proportion of sugar in the different specimens of urine ranged between 19·71 and 31·29 grs. per ounce. The gluten bread that was taken, was eaten, a portion at 10 a.m., and the remainder at 1 p.m. On the three previous days the

gluten bread and bran biscuits were commenced with the patient's breakfast, at 6 a.m. Now, it is interesting to notice that to-day there is a successive fall in the sugar passed during the first three periods, and that it is not till between 1 and 5 p.m. that the rise takes place; whilst on the three preceding days, in each case, there is a diminution from the first to the second period, but at the third, viz., between 9 a.m. and 1 p.m., a rise takes place. As there were four hours' difference in the time at which the vegetable aliment was commenced in the day, so there were four hours' difference in the time at which the escape of sugar began to show an increase.

Weight of the patient to-day, 9 st. 10 lbs., which is the same as when he last weighed.

6th.—Diet, one chop, one pound of dressed meat, and three pints of beef tea. The quantity of sugar showed a descent to 751 grs., and the urine to $52\frac{3}{4}$ oz. The proportion of sugar did not range higher than between 11·70 and 18·45 grs. per oz. Some of the urine again began to deposit lithates.

The patient expressed himself as already feeling better for the omission of the bran biscuits and gluten bread.

7th.—Diet, the same as yesterday. Quantity of sugar passed, 1188 grs.; the proportion of sugar in the six specimens ranging between 9·39 and 18·93 grs. per oz. The quantity of urine was $82\frac{1}{4}$ oz.

8th.—A return to gluten bread was made, in order to determine as clearly as possible its comparative merit, as placed by the side of the bran biscuit. Fourteen ounces of gluten bread were taken, with three chops and three pints of beef tea. The quantity of sugar immediately rose to 3443 grains for the day. The amount of urine was $129\frac{1}{4}$ oz. The urine passed between 1 and 5 a.m. contained 12·96 grs. of sugar to the ounce. The gluten bread was commenced at 6 a.m., and the urine passed between 5 and 9 a.m. contained 28·23 grains of sugar to the ounce. The proportion continued high throughout the remainder of the day.

9th.—Diet, fourteen ounces of gluten bread, one chop, one pound of dressed meat, and three pints of beef tea. Quantity of sugar passed, 4539 grs.; quantity of urine, $172\frac{3}{4}$ oz. The proportion of sugar fluctuated between 23·22 and 30·63 grs.

per ounce. All the specimens of urine were pale and diabetic looking. The patient experienced much thirst and dryness of skin.

On reverting to the effect of gluten bread, as compared with bran biscuit, we observe that when, on March 2nd, seventeen ounces and a half of bran biscuits were consumed, the quantity of sugar passed was 2630 grs. On the following day, twelve ounces were taken, and the amount of sugar was 3291 grs. March 4th, gluten bread was substituted in equal quantity for the bran biscuit; the sugar passed was 4079 grs. March 5th, only five ounces and a half of gluten bread were given; the sugar was still 3228 grs. Two days of animal diet were allowed to intervene for the sugar to fall, and gluten bread was again given to the extent of fourteen ounces each day for a couple of days. On the first day the rise was to 3443 grs., and on the second to 4539. These figures would certainly seem to indicate bran biscuit as less objectionable in the way of producing sugar than gluten bread.

10th.—Diet, one chop, one pound of dressed meat, and three pints of beef tea. Quantity of sugar passed, 1726 grs.; quantity of urine, $88\frac{1}{4}$ oz. Some of the urine again deposited lithates.

11th.—Diet, the same as yesterday. Quantity of sugar, 994 grs.; quantity of urine, 86 oz. The proportion of sugar per ounce of urine varied between 9·60 and 16·74 grs. Some of the urine deposited lithates.

12th.—Diet, four ounces of rice, one egg, two chops, and three pints of beef tea. Quantity of sugar, 2587 grs.; quantity of urine, 107 oz.

The rice was taken in the form of a pudding, with the egg; half of it at 6 a.m., and the remainder at 10 a.m. No other food was taken till 1 p.m. Between 1 and 5 a.m. the amount of sugar passed was 81 grs., and its proportion per ounce 13·56 grs. Between 5 and 9 a.m. the figures given were 576 grs. and 28·80 grs.; and between 9 a.m. and 1 p.m., 837 grs. and 31·29 grs. After this, the gross amount descended to 375 grs., but the proportion continued the same. In the following period there was a descent in both, and in the last the proportion was down to 13·98 grs. The rice, then,

produced evident effects upon the urine within the period that it was taken; and its influence did not become exhausted until quite the end of the day.

13th.—Diet, one pound of potatoes, one chop, one pound of dressed meat, and three pints of beef tea. Quantity of sugar passed, 2270 grs.; quantity of urine, $102\frac{1}{4}$ oz. The potatoes were eaten for breakfast at 6 a.m., and nothing else was taken till 11. From 1 to 5 in the morning the gross amount of sugar voided was 281 grs., and its proportion 18·45 grs. to the ounce of urine. Between 5 and 9 a.m. the gross amount rose to 794 grs., and the proportion per ounce to 24·81 grs. During the next period the proportion still showed a rise to 31·29 grs., but it afterwards in each period fell until the last, when it stood at 14·52. After the rise mentioned in the gross amount, there was a successive descent throughout the day till 9 in the evening.

In order that a fair comparison may be made between potatoes and food of a dry description, one pound of potatoes was well dried, and it was found that it lost eleven ounces in weight. The sixteen ounces, then, only contained five ounces of solid matter.

The patient's weight to-day was 9 st. 13 lbs., being a gain of three pounds since March 5th.

March 14th.—Diet, two ounces of arrowroot, one chop, sixteen ounces of dressed meat, and three pints of beef tea. One ounce of the arrowroot was taken at 6 a.m., and the other ounce at 9 a.m., with no other food till 11. Quantity of sugar passed, as nearly as possible, the same as yesterday, viz., 2278 grs.; quantity of urine also nearly the same, viz. 101 oz. It will be observed that the arrowroot was taken at 6 and 9 a.m., instead of altogether at 6 a.m., as with the potatoes. The rise of sugar for the several periods was, in accordance, less rapid. The figures, given in their order for the six specimens throughout the twenty-four hours, run thus—219, 360, 458, 540, 372, and 329 grs. The proportion of sugar to the ounce of urine in the several specimens was 18·21, 30, 32·70, 25·71, 24·81, and 12·18 grs.

15th.—Diet, four ounces of bread, three chops, and three pints of beef tea. Quantity of sugar, 3521 grs.; quantity of

urine, 131 oz.. The four ounces of bread were taken at 6 a.m. It did not appear to manifest its full effect upon the urine so rapidly as the other articles from the vegetable kingdom that had been given; neither did the effect, when produced, pass off so rapidly. The day began with the patient's passing 197 grs. of sugar during the first four hours, the urine containing 11·79 grs. of sugar to the ounce. From 5 to 9 a.m. the gross amount of sugar was 387 grs., and the proportion 27·15 grs. per ounce. From 9 a.m. to 1 p.m. the figures given were 800 and 31·98; from 1 to 5 p.m., 790 and 35·10; from 5 to 9 p.m., 705 and 30; and from 9 p.m. to 1 a.m., 642 and 22·14. It is thus evident that, although the bread was eaten at 6 o'clock in the morning, its influence was strikingly manifest till the end of the day; and, even during the first four hours on the following morning, the amount of sugar was 337 grs., the proportion of sugar to the ounce of urine being 22·50 grs.

As compared with the bran biscuit and gluten bread, the analytical results obtained show that the ordinary bread produces by far a much more prejudicial effect upon the complaint. This will be seen by reverting to the figures, and bearing in mind the difference in the quantity of the articles taken.

16th.—Diet, two pounds of carrots, one chop, one pound of dressed meat, and three pints of beef tea. Quantity of sugar passed, 2371 grs.; quantity of urine, 91 oz. The carrots were taken in equal portions at 6 and 9 a.m., with an exclusion of other food till 11. The figures representing the gross amount of sugar passed during the six periods of the day, with the proportion of sugar to the ounce of urine, stand thus; 337, 22·50; 346, 28·80; 198, 24·81; 443, 27·69; 375, 31·29; 672, 24.

One pound of carrots, on being well dried, left a residue slightly over three and a half ounces.

17th.—To-day the patient was put back upon a full mixed diet, the same as he had at the commencement of the observations on his case. The amount of sugar rose to 8319 grs., and the amount of urine to 257¾ oz.

18th.—Full mixed diet. Quantity of sugar, 8959 grs.; quantity of urine, 255 oz.

19th.—Full mixed diet. Quantity of sugar, 8355 grs.; quantity of urine, 242¼ oz. The amount of sugar per ounce in the various specimens did not fall below 30·75 grs., nor the sp. gr. below 1031.

Weight of the patient to-day, 9 st. 12 lbs., which is one pound less than when he last weighed on the 13th instant.

20th.—Full mixed diet. Quantity of sugar, 8965 grs.; quantity of urine, 247 oz. Taking the six specimens of urine, the amount of sugar per ounce varied between 33·65 and 39·30 grs., and the sp. gr. between 1036 and 1041.

The patient had now been four days on the ordinary full mixed diet of the hospital, and the amount of sugar he passed had fluctuated between 8319 and 8965 grs. *per diem.* This is not so much, although the diet was the same as when his case was commenced with; for then, during the three days given, the sugar was 8961 grs. at its lowest, and 10573 grs. at its highest. Such a result would tend to show that some permanent improvement, as well as a temporary alleviation, had been obtained by the reduction of sugar for a time through the alteration of diet.

It is curious to notice the successive rise and fall that has occurred in the amount of sugar passed during one day and another under the mixed diet; and, also under the purely animal diet, when the opportunity was afforded, by its continuance for a few consecutive days together. This is rendered strikingly apparent on referring to the plan (opposite p. 140) that gives a general view of the case, and directing the attention to that part of it corresponding to the first six and the last seven days.

21st.—Diet, one chop, one pound of dressed meat, and three pints of beef tea. An immediate fall was observed in the amount of sugar, from 8965 to 1962 grs., and in the amount of urine from 247 to 80 oz.

22nd.—Diet, three chops and three pints of beef tea. Quantity of sugar passed, 1330 grs.; quantity of urine, 84 oz. One of the specimens of urine threw down a lithate deposit.

23rd.—Diet, one chop, one pound of dressed meat, and three

pints of beef tea. Quantity of sugar, 2113 grs.; quantity of urine, 97¾ oz.

Having thus ascertained that through the influence of a strictly animal regimen the elimination of sugar by my patient was so materially reduced, I now resolved to keep him upon animal food, and try if I could discover any medicine that would still further produce a reduction. With this view the patient remained in the hospital under my care till May 31st. I at first administered large doses of the phosphoric acid, then the carbonate of soda, and afterwards the tartrate of potash and soda. From neither, can I say, I obtained evidence of what would enable me to speak of as direct influence, either one way or the other. It is worthy of remark that, although at one time as much as an ounce a day of the tartrate of potash and soda was being administered, yet the urine remained decidedly acid.

Under the animal diet that was in this way continued the patient strikingly improved in bodily appearance, health, and strength. He did not object to his kind of food, did not get tired of it, and said that he felt no longing after bread nor any other vegetable substance. He was allowed greens, however, as it had been observed that they produced no ill effect. Before he left the hospital he expressed himself as feeling well in every respect. He had never, he said, felt better in his life. He had no thirst, no dryness of skin, and his appetite was easily satisfied. He looked plump in the face as compared with what he had previously done, and wore a happy expression upon his countenance in the place of that abject, pinched-up, miserable, downcast look which he once had, and which is so characteristic of the inveterate form of diabetes. He had been gaining flesh, as is shown by the following return of his weight. The last weight mentioned is for March 19th, when it was 9 st. 12 lbs. April 2nd, he weighed 10 st. 1 lb.; April 16th, 10 st. 4 lbs.; April 23rd, 10 st. 8 lbs.; April 30th, 10 st. 10 lbs.; and when he left the hospital, May 31st, 10 st. 8 lbs. He told me he had reached 11 st., but during the hot weather of the last three weeks in May, he lost 6 lbs. The following is a return of the state of his urine during the last four days he remained in the hospital. He was then upon meat, beef tea, greens,

and brandy and soda water, and without medicine of any description. The return shows that the amount of sugar continued at about the same as what had been noticed under similar circumstances before.

Date.	Quantity of urine in oz.	Sp. gr.	Amount of sugar per oz. of urine in grs.	Total amount of sugar per diem in grs.
May 28th ...	62	1035	18·	1116
„ 29th ...	54	1033	17·76	959
„ 30th ...	58	1034	19·44	1127
„ 31st ...	56	1036	18·	1008

Table of the results obtained under the ingestion of various articles of food in the case of JOSEPH NORTH, *æt.* 32, *a diabetic.*

Date.	Period of day.	Sp. gr. of urine.	Quantity of urine in ozs.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar in the urine per 4 hrs. in grs.	Quantity of sugar in the urine per 24 hours in grains.	Diet taken, with 4 ozs. of brandy and 2 bottles of soda water daily, and 1 gr. of opium three times a day. Tea (without sugar), and water ad libitum.	Remarks.
February 1st	1 to 5 p.m.	1045	50	42·36	2118	9475	12·30 p.m., $1\frac{1}{2}$ pint soup (meat and vegetable), bread 2 oz. 4 p.m., bread 5 oz., 1 egg, butter $\frac{1}{2}$ oz. 8 p.m., 1 egg, milk $\frac{1}{2}$ pint, cocoa 10 p.m., 3 oranges 6 a.m., bread 5 oz., milk $\frac{1}{2}$ pint, cocoa, butter $\frac{1}{2}$ oz. 10 a.m., meat 8 oz., bread 4 oz.	All the specimens of urine were very pale-coloured, except that from 1 to 5 a.m., which was nearly of a natural yellow colour.
	5 to 9 p.m.	1041	61	42·	2562			
	9 to 1 a.m.	1040	49	35·10	1720			
	1 to 5 a.m.	1042	29	42·	1218			
February 2nd	5 to 9 a.m.	1039	24	42·36	1017			
	9 to 1 p.m.	1045	20	42·	840			
February 5th	1 to 5 a.m.	1042	14	42·36	593	10573	6 a.m., bread 8 oz., butter $\frac{1}{2}$ oz., milk $\frac{1}{2}$ pint, cocoa 11 a.m., bread 1 oz., meat 4 oz., 1 orange 1 p.m., $1\frac{1}{2}$ pint soup (meat and vegetable) 2 p.m., 1 orange 4 p.m., bread 7 oz., butter $\frac{1}{2}$ oz., 1 egg, milk $\frac{1}{2}$ pint, cocoa 6 p.m., 1 orange 8 p.m., 1 egg, milk $\frac{1}{2}$ pint 9 p.m., 1 orange	The specimen of urine from 1 to 5 a.m. was of nearly a natural yellow colour, that from 5 to 9 a.m. also presented a slight yellow tinge, but all the remaining specimens were nearly as colourless as water.
	5 to 9 a.m.	1039	21	42·36	889			
	9 to 1 p.m.	1035	47	43·62	2050			
	1 to 5 p.m.	1039	$53\frac{1}{2}$	40·	2140			
	5 to 9 p.m.	1034	61	40·70	2482			
	9 to 1 a.m.	1036	$62\frac{1}{2}$	38·70	2419			

February 6th	1 to 5 a.m.	1041	26	42·10	1094	8961	6 a.m., bread 8 oz., butter ½ oz., 1 egg 11 a.m., bread 4 oz., meat 2 oz. 1 p.m., 1½ pint soup (meat and vegetable), bread 2 oz. 4 p.m., bread 8 oz, 1 egg, milk ½ pint, butter ½ oz. 8 p.m., 1 egg, milk ½ pint, cocoa	The specimen of urine from 1 to 5 a.m. was of nearly a natural yellow colour, that from 5 to 9 a.m. also presented some amount of colour; the remaining specimens were diabetic looking.
	5 to 9 a.m.	1041	21½	46·10	991			
	9 to 1 p.m.	1040	30½	47·	1433			
	1 to 5 p.m.	1040	36	45·30	1631			
	5 to 9 p.m.	1035	64	41·	2624			
	9 to 1 a.m.	1039	27 ¾	42·80	1188			
February 7th	1 to 5 a.m.	1043	14	44·80	627	2474	6 a.m., 1 mutton chop (weight 7 oz.) 9 a.m., beef tea 1 pint 12·30 p.m., 1 chop 6 p.m., dressed meat 3 oz. 7·30 p.m., beef tea 1 pint	The specimen of urine from 5 to 9 a.m. was of a natural yellow colour, a shade paler than this, came that from 9 a.m. to 1 p.m., and next, that from 1 to 5 a.m.; but all the specimens were pretty fairly coloured. The urine passed between 1 and 5 p.m. threw down a considerable amount of pale lithate deposit.
	5 to 9 a.m.	1046	12	41·30	495			
	9 to 1 p.m.	1042	9	28·20	254			
	1 to 5 p.m.	1046	7½	28·90	217			
	5 to 9 p.m.	1035	20	29·20	584			
	9 to 1 a.m.	1025	14¾	20·15	297			
February 8th	1 to 5 a.m.	1032	14	23·05	323	3035	6 a.m., one chop 9 a.m., beef tea 1 pint 12·30 p.m., 1 chop, beef tea 1 pint 6 p.m., 1 chop 8 p.m., beef tea 1 pint	All the specimens of urine were pretty fairly coloured, but that passed between 5 and 9 a.m. was more highly coloured than the rest. The urine from 5 to 9 p.m. presented the least amount of colour. That belonging to 1 to 5 p.m. threw down a deposit of lozenge-shaped crystals of lithic acid.
	5 to 9 a.m.	1037	9	26·35	237			
	9 to 1 p.m.	1023	29½	24·40	709			
	1 to 5 p.m.	1030	23	24·	552			
	5 to 9 p.m.	1025	42½	20·65	878			
	9 to 1 a.m.	1013	25	13·45	336			
February 9th	1 to 5 a.m.	1029	11	23·05	254	2471	6 a.m., 1 chop 9 a.m., beef tea 1 pint 12·30 p.m., 1 chop, beef tea 1 pint 4 p.m., 1 egg 7 p.m., dressed meat 4 oz. 8 p.m., beef tea 1 pint	The specimen of urine from 5 to 9 a.m. was the highest coloured, and that from 9 p.m. to 1 a.m. the palest.
	5 to 9 a.m.	1030	15	20·65	310			
	9 to 1 p.m.	1029	21¾	21·80	474			
	1 to 5 p.m.	1030	21½	24·45	526			
	5 to 9 p.m.	1025	26	20·	520			
	9 to 1 a.m.	1013	32	12·10	387			

Date.	Period of day.	Sp. gr. of urine.	Quantity of urine in ozs.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar in the urine per 4 hrs. in grs.	Quantity of sugar in the urine per 24 hours in grains.	Diet taken, with 4 ozs. of brandy and 2 bottles of soda water daily, and 1 gr. of opium three times a day. Tea (without sugar), and water ad libitum.	Remarks.
February 10th	1 to 5 a.m.	1023	15	19·35	290	3179	6 a.m., 1 chop 9 a.m., beef tea 1 pint 1 p.m., meat 5 oz. 4 p.m., 1 pint prepared cocoa sweetened with sugar, 1 egg 6 p.m., beef tea 1 pint 7 p.m., meat 6 oz. 8 p.m., meat 5 oz., beef tea 1 pint	The specimen of urine from 1 to 5 a.m. was the highest coloured; those from 5 to 9 p.m. and 9 p.m. to 1 a.m. were almost destitute of colour. * It is to be remarked, that the patient to-day departed from his instructions, in partaking of cocoa, sweetened with sugar, at 4 p.m.
	5 to 9 a.m.	1039	5	33·30	166			
	9 to 1 p.m.	1039	15	30·75	461			
	1 to 5 p.m.	1022	27	17·35	468			
	5 to 9 p.m.	1026	47	27·90	*1311			
	9 to 1 a.m.	1018	27	17·90	483			
February 11th	1 to 5 a.m.	1027	$35\frac{1}{2}$	21·40	760	3329	6 a.m., 1 chop 8 a.m., beef tea 1 pint 1 p.m., meat 7 oz. 2 p.m., beef tea 1 pint. 4 p.m., meat 2 oz. 7 p.m., meat 4 oz. 8 p.m., beef tea 1 pint	The specimen of urine from 5 to 9 a.m. was the highest coloured; those from 1 to 5 p.m. and 9 p.m. to 1 a.m. presented very little colour.
	5 to 9 a.m.	1033	$13\frac{3}{4}$	26·65	366			
	9 to 1 p.m.	1032	$18\frac{1}{2}$	30·75	569			
	1 to 5 p.m.	1035	$30\frac{1}{2}$	33·30	1016			
	5 to 9 p.m.	1036	15	27·90	418			
	9 to 1 a.m.	1013	27	7·40	200			
February 12th	1 to 5 a.m.	1032	15	19·35	290	1096	6 a.m., 1 pair small soles 10 a.m., beef tea 1 pint 12·30 p.m., 1 chop 4 p.m., meat 4 oz. 6 p.m., beef tea 1 pint 8 p.m., beef tea 1 pint	The specimen of urine from 5 to 9 a.m. was the highest coloured, and that from 9 p.m. to 1 a.m. the palest. A deposit of lithates occurred in the urine passed between 9 a.m. and 1 p.m. The specimens from 1 to 5 p.m., 5 to 9 p.m., and 9 p.m. to 1 a.m., threw down a crystalline deposit, which proved to consist of lithic acid.
	5 to 9 a.m.	1035	$8\frac{3}{4}$	19·35	169			
	9 to 1 p.m.	1033	$13\frac{1}{2}$	16·20	219			
	1 to 5 p.m.	1022	17	7·40	126			
	5 to 9 p.m.	1027	9	12·75	115			
	9 to 1 a.m.	1013	$29\frac{1}{2}$	6·	177			

February 13th	1 to 5 a.m.	1022	27½	12·75	351	1696	6 a.m., 1 pair small soles	The specimen of urine from 5 to 9 a.m. was the highest coloured, and that from 9 p.m. to 1 a.m. the palest. A crystalline deposit of lithic acid occurred in the urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m. * The sp. gr. of this specimen inadvertently escaped being taken.
	5 to 9 a.m.	1030	20½	15·15	311		10·30 a.m., 1 pair small soles	
	9 to 1 p.m.	1025	17	12·60	214		12·30 p.m., 1 pair small soles	
	1 to 5 p.m.	1030	26	14·45	376		1 p.m., beef tea 1 pint	
	5 to 9 p.m.	1019	23	6·85	158		4 p.m., 1 small sole	
	9 to 1 a.m.	*—	49	5·83	286		6 p.m., 1 small sole	
							8 p.m., beef tea 1 pint	
February 14th	1 to 5 a.m.	1026	17½	13·	227	952	6 a.m., jelly 1 pint[1]	The specimen of urine from 5 to 9 a.m. was the highest coloured, that from 5 to 9 p.m. scarcely coloured at all.
	5 to 9 a.m.	1025	12½	13·15	164		9 a.m., jelly 1 pint[1]	
	9 to 1 p.m.	1031	10	25·25	252		1 p.m., meat 10 oz.	
	1 to 5 p.m.	1025	13	9·	117		6 p.m., meat 6 oz.	
	5 to 9 p.m.	1011	27½	3·33	92		8 p.m., beef tea 1 pint	
	9 to 1 a.m.	1013	27	3·69	100			
February 15th	1 to 5 a.m.	1022	16½	4·61	76	569	6 a.m., jelly 1 pint[2]	The specimen of urine from 5 to 9 a.m. was high coloured, and the others moderately coloured, except that from 9 p.m. to 1 a.m., which was pale. The patient weighed to-day 9 st. 5 lbs.
	5 to 9 a.m.	1027	4	7·56	30		10 a.m., jelly 1 pint[2]	
	9 to 1 p.m.	1010	24	2·35	56		1 p.m., 1 chop, beef tea 1 pint	
	1 to 5 p.m.	1015	18¼	4·70	86		8 p.m., 1 chop, beef tea 1 pint	
	5 to 9 p.m.	1022	9	7·50	67			
	9 to 1 a.m.	1015	36	7·05	254			
February 16th	1 to 5 a.m.	1029	11½	12·97	149	1198	6 a.m., milk ½ pint	The specimens of urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m., were pretty fairly coloured; that from 5 to 9 p.m. was pale.
	5 to 9 a.m.	1030	9	16·35	147		9 a.m., 1 chop	
	9 to 1 p.m.	1032	6½	15·63	102		1 p.m., meat 8 oz., milk 1 pint	
	1 to 5 p.m.	1030	14½	15·	217		4 p.m., milk ½ pint	
	5 to 9 p.m.	1012	38	7·65	291		8 p.m., meat 8 oz., milk 1 pint	
	9 to 1 a.m.	1020	26	11·25	292			

[1] These 2 pints of jelly were flavoured with 2 ozs. of sugar, a little sherry and essence of lemon.
[2] This jelly was without sugar, but with the sherry and essence of lemon.

Date.	Period of day.	Sp. gr. of urine.	Quantity of urine in ozs.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar in the urine per 4 hrs. in grs.	Quantity of sugar in the urine per 24 hours in grains.	Diet taken, with 4 ozs. of brandy, and 2 bottles of soda water daily, and 1 gr. of opium three times a day. Tea (without sugar), and water ad libitum.	Remarks.
February 17th	1 to 5 a.m.	1031	8	17·76	142	1258	6 a.m., 1 chop	The specimens of urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m., were moderately high coloured; that from 5 to 9 p.m. was pale.
	5 to 9 a.m.	1040	7	21·48	150		1 p.m., meat 10 oz., milk 1 pint	
	9 to 1 p.m.	1030	5	14·82	74		4 p.m., meat 4 oz., milk 1 pint	
	1 to 5 p.m.	1020	$21\frac{1}{4}$	8 31	177		8 p.m., meat 2 oz., milk 1 pint	
	5 to 9 p.m.	1015	37	9·84	364			
	9 to 1 a.m.	1023	20	17·55	351			
February 18th	1 to 5 a.m.	1030	$9\frac{1}{2}$	18·93	180	1485	6 a.m., 1 chop	The specimens of urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m., were pretty fairly coloured; those from 1 to 5 p.m. and 9 p.m. to 1 a.m. were pale.
	5 to 9 a.m.	1037	$8\frac{1}{4}$	23·58	195		1 p.m., meat 10 oz., milk 1 pint	
	9 to 1 p.m.	1020	$14\frac{1}{2}$	11·07	160		4 p.m., milk 1 pint, with suet $\frac{1}{4}$ lb	
	1 to 5 p.m.	1016	$24\frac{1}{4}$	9·33	226		8 p.m., meat 6 oz., milk 1 pint, with suet $\frac{1}{4}$ lb	
	5 to 9 p.m.	1031	$9\frac{1}{2}$	24·	228			
	9 to 1 a.m.	1020	$33\frac{1}{2}$	14·82	496			
February 19th	1 to 5 a.m.	1030	8	20·55	164	1722	6 a.m., meat 8 oz.	The specimens of urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m., were moderately high coloured; that from 9 p.m. to 1 a.m. was pale.
	5 to 9 a.m.	1035	$7\frac{1}{2}$	22·50	169		1 p.m., 1 chop, milk 1 pint, with suet $\frac{1}{4}$ lb	
	9 to 1 p.m.	1038	9	24·81	223		4 p.m. milk 1 pint, with suet $\frac{1}{4}$ lb	
	1 to 5 p.m.	1032	11	21·81	240		8 p.m., 1 chop, milk 1 pint, with suet $\frac{1}{4}$ lb	
	5 to 9 p.m.	1030	16	22·50	360			
	9 to 1 a.m.	1022	$32\frac{1}{4}$	17·55	566			
February 20th	1 to 5 a.m.	1032	22	24·	528	2225	6 a.m., 1 chop	The specimens of urine from 5 to 9 a.m. and 9 a.m. to 1 p.m. were moderately high coloured; that from 9 p.m. to 1 a.m. was pale.
	5 to 9 a.m.	1040	7	22·50	157		11 a.m., milk 1 pint, with suet $\frac{1}{4}$ lb	
	9 to 1 p.m.	1037	$10\frac{1}{4}$	24·	246		1 p.m., meat 8 oz.	
	1 to 5 p.m.	1035	11	25·71	283		4 p.m., meat 4 oz., milk 1 pint	
	5 to 9 p.m.	1033	$10\frac{1}{4}$	26·64	273		8 p.m., meat 4 oz., milk 1 pint	
	9 to 1 a.m.	1025	41	18·	738			

February 21st	1 to 5 a.m.	1037	9	26·64	240	927	6 a.m., 4 eggs, hard boiled 11 a.m., 2 eggs, hard boiled 1 p.m., meat 8 oz., beef tea 1 pint 4 p.m., meat 4 oz. 8 p.m., meat 4 oz., beef tea 1 pint	The specimens of urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m., were fairly coloured; that from 9 p.m. to 1 a.m. was of a pale yellow colour. The specimens belonging to the three latter periods of the day deposited crystals of lithic acid. Weight of patient 9 st. 5 lbs.
	5 to 9 a.m.	1040	$7\frac{1}{4}$	23·22	168			
	9 to 1 p.m.	1035	$8\frac{1}{2}$	17 55	149			
	1 to 5 p.m.	1032	7	10·74	75			
	5 to 9 p.m.	1033	$5\frac{3}{4}$	12 63	73			
	9 to 1 a.m.	1015	$27\frac{1}{2}$	8·07	222			
February 22nd	1 to 5 a.m.	1027	8	12·51	100	734	6 a.m., 4 eggs, hard boiled 10·30 a.m., 4 eggs, hard boiled 1 p.m., 1 chop, beef tea 1 pint 8 p.m., 1 chop, beef tea 1 pint	The specimens of urine from 5 to 9 a.m., 1 to 5 p.m., and 5 to 9 p.m., were high coloured; that from 9 p.m. to 1 a.m. was pale. The specimen from 9 a.m. to to 1 p.m. was milk white and opaque, from deposition of lithates. The specimens from 1 to 5 a.m and 9 a.m. to 1 p.m. threw down crystals of lithic acid.
	5 to 9 a.m.	1028	$14\frac{1}{2}$	10·74	156			
	9 to 1 p.m.	1027	$5\frac{1}{2}$	6·36	35			
	1 to 5 p.m.	1032	10	7·56	76			
	5 to 9 p.m.	1037	$6\frac{1}{2}$	15·63	102			
	9 to 1 a.m.	1020	$27\frac{1}{4}$	9·72	265			
February 23rd	1 to 5 a.m.	1030	21	17·55	369	1310	6 a.m., oysters 4 doz. 1 p.m., meat 16 oz., beef tea 1 pint 6 p.m., beef tea 1 pint 8 p.m., beef tea 1 pint	The specimens of urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m., were pretty highly coloured; that from 9 p.m. to 1 a.m. was straw-coloured.
	5 to 9 a.m.	1030	14	16·92	237			
	9 to 1 p.m.	1027	14	14·67	205			
	1 to 5 p.m.	1030	15	6·54	98			
	5 to 9 p.m.	1017	$13\frac{3}{4}$	11·07	152			
	9 to 1 a.m.	1012	38	6·54	249			
February 24th	1 to 5 a.m.	1020	28	10·56	296	896	7 a.m., oysters 4 doz. 1 p.m., meat 12 oz, beef tea 1 pint 4 p.m., meat 4 oz. 6·30 p.m., beef tea 1 pint 8 p.m., beef tea 1 pint	The specimen of urine from 5 to 9 a.m. was the highest coloured, and that from 9 a.m. to 1 p.m. the least so. The urine passed between 1 and 5 p.m. threw down a copious deposit of lithates.
	5 to 9 a.m.	1028	9	11·61	104			
	9 to 1 p.m.	1026	10	10·56	106			
	1 to 5 p.m.	1032	$6\frac{1}{2}$	2·4	16			
	5 to 9 p.m.	1025	10	6·15	61			
	9 to 1 a.m.	1015	$33\frac{1}{2}$	9·33	313			

Date.	Period of day.	Sp. gr. of urine.	Quantity of urine in ozs.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar in the urine per 4 hrs. in grs.	Quantity of sugar in the urine per 24 hours in grains.	Diet taken, with 4 ozs. of brandy and 2 bottles of soda water daily, and 1 gr. of opium three times a day. Tea (without sugar), and water ad libitum.	Remarks.
February 25th	1 to 5 a.m.	1027	$17\frac{1}{2}$	15·	262	867	6 a.m., 1 chop 1 p.m., meat 4 oz., beef tea 1 pint 4 p.m., meat 4 oz. 6·30 p.m., beef tea 1 pint 8 p.m., beef tea 1 pint	The specimens of urine from 5 to 9 a.m. presented the highest colour; that from 9 p.m. to 1 a.m. was of a pale straw.
	5 to 9 a.m.	1034	$5\frac{3}{4}$	12·63	73			
	9 to 1 p.m.	1028	9	10·26	92			
	1 to 5 p.m.	1031	13	12·18	158			
	5 to 9 p.m.	1030	9	12·84	116			
	9 to 1 a.m.	1015	24	6·90	166			
February 26th	1 to 5 a.m.	1022	27	12·	324	886	6 a.m., 1 chop 10·30, suet 4 oz. 1 p.m., 1 chop, beef tea 1 pint 4 p.m., suet 4 oz. 7 p.m., beef tea 1 pint 8 p.m., 1 chop, beef tea 1 pint	All the specimens of urine were of a fairish yellow colour, but that from 5 to 9 a.m. was the darkest, and that from 9 p.m. to 1 a.m. the palest.
	5 to 9 a.m.	1031	16	14·4	230			
	9 to 1 p.m.	1025	11	7·80	86			
	1 to 5 p.m.	1019	12	trace of sugar only				
	5 to 9 p.m.	1023	6	6·				
	9 to 1 a.m.	1015	31	6·78	210			
February 27th	1 to 5 a.m.	1026	$16\frac{1}{2}$	12·84	212	1177	6 a.m, 1 chop, gum 4 oz. dissolved in water and drank at intervals during the morning 1 p.m., meat 8 oz. 4 p.m., beef tea 1 pint 7 p.m., meat 8 oz., beef tea 1 pint	The specimen of urine from 1 to 5 a.m. was the highest coloured, that from 5 to 9 p.m. the least so. Weight of patient, 9 st., 10 lbs.
	5 to 9 a.m.	1035	7	15·30	107			
	9 to 1 p.m.	1035	7	16·74	117			
	1 to 5 p.m.	1037	$11\frac{1}{4}$	18·	202			
	5 to 9 p.m.	1023	$33\frac{3}{4}$	9·27	313			
	9 to 1 a.m.	1024	$19\frac{1}{2}$	11·61	226			
February 28th	1 to 5 a.m.	1032	11	15·99	176	887	6 a.m., 1 chop 1 p.m., meat 8 oz., beef tea 1 pint 4 p.m., meat 4 oz. 7 p.m., meat 4 oz., beef tea 1 pint 8 p.m., beef tea 1 pint	The specimens of urine from 1 to 5 a.m. 5 to 9 a.m., and 9 a.m. to 1 p.m., presented the highest colour; that from 9 p.m. to 1 a.m. was of a straw colour. The specimen passed between 1 and 5 p.m. threw down lithates.
	5 to 9 a.m.	1037	3	20·55	62			
	9 to 1 p.m.	1037	$9\frac{1}{2}$	19·44	185			
	1 to 5 p.m.	1037	8	12·63	101			
	5 to 9 p.m.	1021	12	8·31	100			
	9 to 1 a.m.	1017	28	9·39	263			

March 1st	1 to 5 a.m.	1030	12	12·39	149	594	6 a.m., 1 chop 1 p.m., 1 chop, greens 2 p.m., beef tea 1 pint 8 p.m., 1 chop, beef tea 1 pint	The specimen of urine from 1 to 5 a.m. was the highest, and that from 9 p.m to 1 a.m. the least coloured. The specimens from 5 to 9 a.m., 9 a.m. to 1 p.m., 1 to 5 p.m, and 5 to 9 p.m., deposited lithates.
	5 to 9 a.m.	1032	2	7·2	14			
	9 to 1 p.m.	1025	8½	5·79	49			
	1 to 5 p.m.	1024	8	trace of sugar only				
	5 to 9 p.m.	1032	9	13·08	118			
	9 to 1 a.m.	1021	26	10·14	264			
March 2nd	1 to 5 a.m.	1032	19	15·99	304	2630	6 a.m., 1 chop, bran biscuit 3 oz. 10 a.m., bran biscuit 5½ oz. 1 p.m., meat 8 oz., bran biscuit 3 oz., beef tea 1 pint. 4 p.m., meat 4 oz., bran biscuit 3 oz. 6 p.m., beef tea 1 pint 8 p.m., meat 4 oz., bran biscuit 3 oz., beef tea 1 pint	The specimens of urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m., were highish coloured; that from 5 to 9 p.m. was of a straw colour.
	5 to 9 a.m.	1040	8	29·37	235			
	9 to 1 p.m.	1039	14	31·29	438			
	1 to 5 p.m.	1040	15½	32·70	507			
	5 to 9 p.m.	1040	20	30·63	613			
	9 to 1 a.m.	1037	20	26·64	533			
March 3rd	1 to 5 a.m.	1038	19	27·15	516	3291	6 a.m., 1 chop, bran biscuit 3 oz. 10 a.m., bran biscuit 3 oz. 1 p.m., meat 8 oz., bran biscuit 3 oz., beef tea 1 pint 4 p.m., meat 4 oz., bran biscuit 3 oz. 7 p.m., beef tea 1 pint 8 p.m., meat 4 oz., beef tea 1 pint	The specimen of urine from 5 to 9 a.m. was the highest coloured, that from 9 p.m. to 1 a.m. the least coloured.
	5 to 9 a.m.	1041	9	28·80	259			
	9 to 1 p.m.	1042	14¾	33·18	494			
	1 to 5 p.m.	1043	18	36·	648			
	5 to 9 p.m.	1039	25½	32·70	834			
	9 to 1 a.m.	1030	24	22·50	540			
March 4th	1 to 5 a.m.	1037	25½	25·71	656	4079	6 a.m., 1 chop, gluten bread 3 oz. 10 a.m., gluten bread 2 oz. 1 p.m., meat 8 oz., gluten bread 2 oz., beef tea 1 pint 4 p.m., gluten bread 3 oz., beef tea 1 pint 8 p.m., meat 4 oz., gluten bread 2 oz., beef tea 1 pint	The specimens of urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m., were highish coloured; the others were pale.
	5 to 9 a.m.	1037	9¼	28·23	261			
	9 to 1 p.m.	1038	20	30·	600			
	1 to 5 p.m.	1035	25¼	30·63	773			
	5 to 9 p.m.	1031	28	27·69	775			
	9 to 1 a.m.	1032	43	23·58	1014			

Date.	Period of day.	Sp. gr. of urine.	Quantity of urine in ozs.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar in the urine per 4 hrs. in grs.	Quantity of sugar in the urine per 24 hours in grains.	Diet taken, with 4 ozs. of brandy and 2 bottles of soda water daily, and 1 gr. of opium three times a day. Tea (without sugar), and water ad libitum.	Remarks.
March 5th	1 to 5 a.m.	1035	23	25·71	591	3228	6 a.m., 1 chop	The specimens of urine from 5 to 9 a.m. and 9 a.m. to 1 p.m. presented the most colour; that from 1 to 5 p.m. the least. Weight of patient, 9 st. 10 lbs.
	5 to 9 a.m.	1037	17	25·71	437		10 a.m., meat 4 oz., gluten bread 2½ oz.	
	9 to 1 p.m.	1040	11¼	31·29	352		1 p.m., 1 chop, gluten bread 3 oz., beef tea 1 pint	
	1 to 5 p.m.	1037	35	30·	1050		7 p.m., beef tea 1 pint	
	5 to 9 p.m.	1036	17¼	25·71	443		8 p.m., 1 chop, beef tea 1 pint	
	9 to 1 a.m.	1035	18	19·71	355			
March 6th	1 to 5 a.m.	1039	8	18·45	148	751	6 a.m., 1 chop	The specimen of urine from 1 to 5 a.m. was pretty highly coloured, those from 5 to 9 p.m. and 9 p.m. to 1 a.m. were the palest. The specimens from 5 to 9 a.m, 9 a.m. to 1 p.m., and 1 to 5 p.m., deposited lithates.
	5 to 9 a.m.	1037	6	15·	90		1 p.m., meat 8 oz., beef tea 1 pint	
	9 to 1 p.m.	1038	5¾	16·92	97		4 p.m., meat 4 oz., beef tea 1 pint	
	1 to 5 p.m.	1040	7½	11·70	88		7·30 p.m., meat 4 oz., beef tea 1 pint	
	5 to 9 p.m.	1035	6½	14·25	93			
	9 to 1 a.m.	1029	19	12·39	235			
March 7th	1 to 5 a.m.	1035	18¾	15·63	293	1188	6 a.m., 1 chop	The specimens of urine from 1 to 5 a.m., 5 to 9 a.m., and 9 a.m. to 1 p.m., were highish coloured; that from 9 p.m. to 1 a.m. was straw coloured.
	5 to 9 a.m.	1037	10¾	18·93	203		1 p.m., meat 10 oz., beef tea 1 pint	
	9 to 1 p.m.	1035	7	14·52	102		4 p.m., beef tea 1 pint	
	1 to 5 p.m.	1040	10½	18·45	194		7 p.m., meat 6 oz., beef tea 1 pint	
	5 to 9 p.m.	1033	14½	13·83	201			
	9 to 1 a.m.	1021	20¾	9·39	195			
March 8th	1 to 5 a.m.	1032	16	12·96	207	3443	6 a.m., 1 chop, gluten bread 3 oz.	The specimen of urine from 5 to 9 a.m. was highish coloured, the others were pale.
	5 to 9 a.m.	1036	13	28·23	367		10·30 a.m., gluten bread 3½ oz.	
	9 to 1 p.m.	1037	15	31·29	469		1 p.m., 1 chop, gluten bread 3½ oz., beef tea 1 pint	
	1 to 5 p.m.	1037	28	30·	840		4 p.m., gluten bread 2½ oz., beef tea 1 pint	
	5 to 9 p.m.	1035	33	27·69	914		8 p.m., 1 chop, gluten bread 1½ oz., beef tea 1 pint	
	9 to 1 a.m.	1034	24¼	26·64	646			

March 9th	1 to 5 a.m.	1034	33½	25·71	861	4539	6 a.m., 1 chop, gluten bread 2½ oz. 11 a.m., gluten bread 3 oz. 1 p.m., meat 8 oz., gluten bread 3 oz., beef tea 1 pint 4 p.m., gluten bread 3 oz., beef tea 1 pint 8 p.m., meat 8 oz., gluten bread 2½ oz., beef tea 1 pint	All the specimens of urine were of a straw colour, with scarcely any shade of difference between them.
	5 to 9 a.m.	1037	28	27·15	760			
	9 to 1 p.m.	1035	28	25·71	720			
	1 to 5 p.m.	1041	24	30·63	735			
	5 to 9 p.m.	1039	35	25·71	900			
	9 to 1 a.m.	1034	24¼	23·22	563			
March 10th	1 to 5 a.m.	1035	28¼	24·81	701	1726	6 a.m., 1 chop 1 p.m., meat 8 oz., beef tea 1 pint 4 p.m., meat 4 oz., beef tea 1 pint 8 p.m., meat 4 oz., beef tea 1 pint	The specimens of urine from 1 to 5 a.m. and 9 a.m. to 1 p.m. were highish coloured; that from 9 p.m. to 1 a.m. was pale. The urine passed between 5 and 9 a.m. deposited lithates.
	5 to 9 a.m.	1010	4	19·20	77			
	9 to 1 p.m.	1035	10	16·35	163			
	1 to 5 p.m.	1039	11¼	19·44	219			
	5 to 9 p.m.	1032	15¾	15·99	252			
	9 to 1 a.m.	1031	19	16·53	314			
March 11th	1 to 5 a.m.	1033	10	16·74	167	994	6 a.m., 1 chop 1 p.m., meat 8 oz., beef tea 1 pint 4 p.m., meat 8 oz., beef tea 1 pint 7 p.m., beef tea 1 pint	The specimen of urine from 1 to 5 a.m. was highish coloured, that from 5 to 9 p.m. pale. The urine voided between 5 and 9 a.m., 9 a.m. and 1 p.m., and 1 and 5 p.m., threw down lithates.
	5 to 9 a.m.	1037	6	13·83	83			
	9 to 1 p.m.	1032	7½	9·60	72			
	1 to 5 p.m.	1037	12½	10·56	132			
	5 to 9 p.m.	1020	29	9·72	282			
	9 to 1 a.m.	1025	21	12·30	258			
March 12th	1 to 5 a.m.	1032	6	13·56	81	2587	6 a.m., half of a rice pudding composed of 4 oz. of rice and 1 egg 10 a.m., remainder of the rice pudding 1 p.m., 1 chop, beef tea 1 pint 4 p.m., beef tea 1 pint 8 p.m. 1 chop, beef tea 1 pint	The specimen of urine from 1 to 5 a.m. was the highest coloured, that from 9 p.m to 1 a.m. the least coloured.
	5 to 9 a.m.	1033	20	28·80	576			
	9 to 1 p.m.	1035	26¾	31·29	837			
	1 to 5 p.m.	1042	12	31·29	375			
	5 to 9 p.m.	1039	15	22·50	337			
	9 to 1 a.m.	1025	27¼	13·98	381			

Date.	Period of day.	Sp. gr. of urine.	Quantity of urine in ozs.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar in the urine per 4 hrs. in grs.	Quantity of sugar in the urine per 24 hours in grains.	Diet taken, with 4 ozs. of brandy and 2 bottles of soda water daily, and 1 gr. of opium three times a day. Tea (without sugar), and water ad libitum.	Remarks.
March 13th	1 to 5 a.m.	1035	15¼	18·45	281	2270	6 a.m., potatoes 1 lb. (boiled) 11 a.m., 1 chop 1 p.m., meat 8 oz., beef tea 1 pint 4 p.m., meat 4 oz., beef tea 1 pint 8 p.m., meat 4 oz., beef tea 1 pint	The specimen of urine from 1 to 5 a.m. was highish coloured, that from 5 to 9 p.m. was of a palish straw and deposited lithates. Weight of patient, 9 st. 13 lbs.
	5 to 9 a.m.	1032	32	24·81	794			
	9 to 1 p.m.	1010	16	31·29	501			
	1 to 5 p.m.	1014	7½	28·80	216			
	5 to 9 p.m.	1037	11½	16·35	188			
	9 to 1 a.m.	1026	20	14·52	290			
March 14th	1 to 5 a.m.	1037	12	18·21	219	2278	6 a.m., arrowroot 1 oz. in 1 pint of water 9 a.m., arrowroot 1 oz. in 1 pint of water 11 a.m., 1 chop 1 p.m., meat 12 oz., beef tea 1 pint 4 p.m., meat 4 oz., beef tea 1 pint 7 p.m. beef tea 1 pint	The specimen of urine from 1 to 5 a.m. was high coloured, that fron 9 p.m. to 1 a.m. was pale.
	5 to 9 a.m.	1038	12	30·	360			
	9 to 1 p.m.	1037	14	32·70	458			
	1 to 5 p.m.	1041	21	25·71	540			
	5 to 9 p.m.	1035	15	24·81	372			
	9 to 1 a.m.	1021	27	12·18	329			
March 15th	1 to 5 a.m.	1030	16¾	11·79	197	3521	6 a.m., 1 chop, bread 4 oz. 1 p.m., 1 chop, beef tea 1 pint 4 p.m., beef tea 1 pint 7·30 p.m., 1 chop, beef tea 1 pint	The specimens of urine from 1 to 5 a.m. and 5 to 9 a.m. were of a highish colour, those from 1 to 5 p.m. and 9 p.m. to 1 a.m. were pale.
	5 to 9 a.m.	1032	14¼	27·15	387			
	9 to 1 p.m.	1034	25	31·98	800			
	1 to 5 p.m.	1037	22½	35·10	790			
	5 to 9 p.m.	1034	23½	30·	705			
	9 to 1 a.m.	1033	29	22·14	642			
March 16th	1 to 5 a.m.	1040	15	22·50	337	2371	6 a.m., carrots 1 lb. (boiled) 9 a.m., carrots 1 lb. (boiled) 11 a.m., 1 chop 1 p.m., meat 12 oz, beef tea 1 pint 4 p.m., meat 4 oz., beef tea 1 pint 8 p.m., beef tea 1 pint	The specimen of urine from 1 to 5 a.m. was highish coloured, those from 1 to 5 p.m., 5 to 9 p.m., and 9 p.m. to 1 a.m., were pale.
	5 to 9 a.m.	1038	12	28·80	346			
	9 to 1 p.m.	1037	8	24·81	198			
	1 to 5 p.m.	1037	16	27·69	443			
	5 to 9 p.m.	1036	12	31·29	375			
	to 1 a.m.	1031	28	24·	672			

March 17th	1 to 5 a.m. 5 to 9 a.m. 9 to 1 p.m. 1 to 5 p.m. 5 to 9 p.m. 9 to 1 a.m.	1033 1032 1030 1039 1032 1032	17¼ 26½ 60 38 51 65	23·58 30· 30· 37·89 31·29 35·10	407 795 1800 1440 1596 2281	8319	6 a.m., bread 8 oz., butter ½ oz. 10 a.m., bread 5 oz., milk ½ pint 1 p.m., meat 5 oz., greens 3 p.m., suet pudding composed of suet, 1 egg, and a little flour; 2 oranges 4 p.m., tea with ½ pint milk and sugar 6 p.m., meat 4 oz., bread 4 oz., butter ½ oz. 7 p.m., 1 orange	The specimen of urine from 1 to 5 a.m. was pretty highly coloured, and next to it was that from 5 to 9 a.m. All the remaining specimens were exceedingly pale.
March 18th	1 to 5 a.m. 5 to 9 a.m. 9 to 1 p.m. 1 to 5 p.m. 5 to 9 p.m. 9 to 1 a.m.	1040 1038 1035 1037 1034 1035	32 30 65 54 29 45	36· 36· 34·26 34·75 35·80 35·25	1152 1080 2227 1876 1038 1586	8959	6 a.m., meat 4 oz., bread 4 oz., butter ½ oz., tea with sugar 10 a.m., meat 4 oz., bread 5 oz., 1 orange 11 a.m., milk 1 pint 1 p.m., meat 4 oz., greens 6 oz. 3 p.m., 1 orange 4 p.m., bread 5 oz., butter ½ oz., tea with sugar 7 p.m., 1 orange	All the specimens of urine were exceedingly pale in colour.
March 19th	1 to 5 a.m. 5 to 9 a.m. 9 to 1 p.m. 1 to 5 p.m. 5 to 9 p.m. 9 to 1 a.m.	1040 1042 1039 1039 1032 1031	31 28¼ 31 57 46 49	33·30 36·90 36·35 36·35 34·25 30·75	1032 1042 1127 2072 1575 1507	8355	6 a.m., bread 4 oz., butter ½ oz., 1 pint cocoa with sugar 11·30 a.m., bread 5 oz., butter ¼ oz., milk ½ pint 1 p.m., soup 1 pint (meat and vegetable), bread 3 oz. 2 p.m., milk ½ pint 3 p.m., 1 orange 4 p.m., bread 4 oz., butter ¼ oz. tea with sugar	All the specimens of urine were exceedingly pale coloured, but that from 1 to 5 a.m. was the highest. Weight of patient, 9 st. 12 lbs.

Date.	Period of day.	Sp. gr. of urine.	Quantity of urine in ozs.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar in the urine per 4 hrs. in grs.	Quantity of sugar in the urine per 24 hours in grains.	Diet taken, with 4 ozs. of brandy and 2 bottles of soda water daily, and 1 gr. of opium three times a day. Tea (without sugar), and water ad libitum.	Remarks.
March 20th	1 to 5 a.m. 5 to 9 a.m. 9 to 1 p.m. 1 to 5 p.m. 5 to 9 p.m. 9 to 1 a.m.	1041 1039 1038 1040 1036 1038	44 31 39 43 49 41	39·30 39·30 37·50 34·25 34·75 33·65	1729 1218 1462 1473 1703 1380	8965	6 a.m., bread 4 oz., butter ½ oz., tea with sugar 10 a.m., bread 4 oz., meat 3 oz., milk ½ pint 1 p.m., meat 4 oz., greens, milk ½ pint 2 p.m., 1 orange 4 p.m., bread 4 oz., butter ½ oz., tea with sugar 7 p.m., 1 orange 8 p.m., meat 2 oz., bread 3 oz.	All the specimens of urine were exceedingly pale coloured.
March 21st	1 to 5 a.m. 5 to 9 a.m. 9 to 1 p.m. 1 to 5 p.m. 5 to 9 p.m. 9 to 1 a.m.	1042 1048 1045 1045 1037 1026	18 7 6 10 17 22	38·70 30· 19·71 23·22 23·58 13·83	697 210 118 232 401 304	1962	6 a.m., 1 chop 1 p.m., meat 8 oz., beef tea 1 pint 4 p.m., meat 4 oz., beef tea 1 pint 8 p.m., meat 4 oz., beef tea 1 pint	The specimens of urine from 1 to 5 a.m. and 5 to 9 a.m. were moderately coloured, the other specimens were pale.
March 22nd	1 to 5 a.m. 5 to 9 a.m. 9 to 1 p.m. 1 to 5 p.m. 5 to 9 p.m. 9 to 1 a.m.	1036 1037 1035 1035 1027 1023	10 7 6 15 18 28	15·81 18· 11·25 17·76 19·44 12·96	158 126 67 266 350 363	1330	6 a.m., 1 chop 1 p.m., 1 chop, beef tea 1 pint 4 p.m., beef tea 1 pint 8 p.m., 1 chop, beef tea 1 pint	The specimen of urine from 1 to 5 a.m. was pretty highly coloured, that from 9 p.m. to 1 a.m. was pale. The urine passed between 9 a.m. and 1 p.m. threw down a copious deposit of lithates.
March 23rd	1 to 5 a.m. 5 to 9 a.m. 9 to 1 p.m. 1 to 5 p.m. 5 to 9 p.m. 9 to 1 a.m.	1032 1033 1037 1038 1035 1034	24 8 12 16¼ 12 25½	18· 17·76 22·50 26·16 20·28 23·58	432 142 270 425 243 601	2113	6 a.m., 1 chop 1 p.m., meat 8 oz. 2 p.m., beef tea 1 pint 4 p.m., beef tea 1 pint 8 p.m., meat 8 oz., beef tea 1 pint	The specimen of urine passed between 5 and 9 a.m. was the highest coloured.

CASE II.

George P—, æt. 23, admitted under my care, with diabetes, into Stephen Ward, February 6th, 1861. A deaf and dumb person, with the complaint of a year's duration. After admission into the hospital he was directed to preserve all the urine he passed, which was collected every twenty-four hours, measured, and analysed: the following is a detailed account of the state of it as long as the examination was made. It will be observed, that during the first nine days he was under an ordinary full mixed diet, and that after this period his food was changed:

Date.		Quantity of urine. Pints.	Oz.	Sp. gr. of urine.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar per 24 hours in grs.	Diet and medicine.
February	11th	13	,,	1038	44·40	11544	Ordinary full mixed diet. Inf. Gentianæ co. ℥j ter die.
,,	12th	12	10	1037	45·25	11312	
,,	14th	21	,,	1039	47·05	19761	Ordinary full mixed diet. Pot. Acet. ʒss per diem ex Aquâ.
,,	15th	20	,,	1036	37·50	15000	
,,	16th	17	,,	1036	37·50	12750	
,,	18th	18	,,	1039	40·	14400	
,,	19th	18	,,	1039	42·58	15329	
,,	20th	20	,,	1035	37·50	15000	
,,	21st	17	10	1038	40·	14000	
,,	22nd	4	16	1036	28·55	2741	Animal diet, consisting of 1 chop, 1 ℔ of dressed meat, and 3 pints of beef tea per diem, with greens. Sherry ℥vj. Pot. Acet. ʒss per diem ex Aquâ.
,,	23rd	5	2	1027	15·35	1566	
,,	24th	6	,,	1025	10·74	1289	
,,	25th	5	,,	1025	14·40	1440	
,,	26th	7	2	1022	10·05	1427	
,,	27th	5	4	1020	5·31	552	
,,	28th	5	4	1020	6·64	691	
March	1st ...	4	15	1024	12·63	1199	
,,	2nd...	5	2	1023	11·79	1202	
,,	3rd...	4	12	1026	13·83	1272	Animal diet as above, without any medicine.
,,	4th...	3	11	1030	16·92	1201	
,,	5th...	5	6	1025	13·08	1386	
,,	6th...	4	,,	1027	11·61	928	Animal diet as above, with Succi Limonum ℥j, Tr. Opii ♏viij, 4tis horis.
,,	7th...	4	2	1025	12·	984	
,,	8th...	5	2	1025	13·83	1410	
,,	9th...	3	18	1025	11·52	898	
,,	10th...	4	18	1025	12·	1176	
,,	11th...	4	17	1022	9·84	954	
,,	12th...	5	2	1027	12·	1224	
,,	13th...	5	1	1031	15·30	1545	
,,	14th...	4	16	1032	17·13	1644	
,,	15th...	4	3	1030	14·25	1182	
,,	16th...	4	16	1031	15·15	1454	

Date.	Quantity of urine Pints.	Oz.	Sp. gr. of urine.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar per 24 hours in grs.	Diet and medicine.
March 17th...	4	16	1030	14·40	1382	Animal diet as above, with Sodæ Potassio-tartratis ℥ss per diem ex Aquâ.
„ 18th...	4	13	1033	15·63	1453	
„ 20th...	4	2	1025	11·20	918	
„ 21st ...	4	6	1030	12·39	1065	
„ 22nd...	4	7	1031	13·08	1138	
„ 23rd...	4	„	1030	11·79	943	
„ 24th...	3	3¾	1030	10·56	673	Animal diet as before, with Sodæ Potassio-tartratis ʒvj per diem ex Aquâ, and Pulv. Opii gr. j nocte maneque.
„ 25th...	3	2	1026	10·05	623	
„ 26th...	3	13	1029	10·50	766	
„ 27th...	4	11	1027	8·88	808	
„ 28th...	4	„	1029	9·45	756	Animal diet as before, with Sodæ Potassio-tartratis ℥j per diem ex Aquâ, and Pulv. Opii gr. j nocte maneque.
„ 29th...	4	8	1030	10·56	937	
„ 30th...	5	8¼	1030	12·84	1390	
„ 31st ...	5	6	1031	12·	1272	
April 1st ...	5	5	1033	12·39	1301	
„ 2nd ...	4	„	1032	11·79	943	
„ 3rd ...	5	„	1028	9·99	999	
„ 4th ...	5	12	1032	13·56	1518	Animal diet as before, with Acidi Phosphorici dil. ℥j per diem ex Aquâ.
„ 5th ...	5	7	1028	10·89	1165	
„ 6th ...	5	„	1035	16·35	1635	
„ 7th ...	4	16	1036	17·55	1684	
„ 8th ...	4	16	1035	15·99	1535	
„ 9th ...	4	„	1035	15·99	1279	
„ 10th ...	4	„	1040	20·55	1644	
„ 11th ..	4	6	1032	15·99	1375	
„ 12th ...	4	„	1031	15·30	1224	
„ 13th ...	4	10	1033	13·56	1220	
„ 14th ...	4	4	1032	14·40	1209	
„ 15th ...	4	4	1031	14·52	1219	
„ 16th ...	4	5	1032	14·40	1224	
„ 17th ...	5	6	1034	18·	1908	
„ 18th ...	4	12	1031	14·40	1324	
„ 19th ...	3	16	1031	14·40	1094	
„ 20th ...	5	14	1034	15·	1710	

On casting the eye through the preceding table it will be noticed that at one time after admission, before the diet was changed, the patient eliminated with his urine, which measured 21 pints in quantity, no less an amount than upwards of 19,000 grs. of sugar during the twenty-four hours. 7000 grains making the avoirdupois pound, the amount actually passed—19,761 grs.—gives upwards of 2¾lbs. of sugar for the day.

February 21st, the last day on which the mixed diet was taken, the quantity of urine passed was 17½ pints, and the quantity of sugar 14,000 grs. or exactly 2 lbs. On the

following day, through the change of diet, the urine fell to four pints, 16 ozs., and the sugar to 2741 grs. The sugar always afterwards continued considerably below this, and on one day, February 27th, descended to as low as 552 grs., which is but little more than an ounce.

I cannot vouch that this patient always strictly adhered to the diet prescribed him. Being deaf and dumb, and his intelligence limited, it was difficult to make him fully appreciate the necessity of a total abstinence from vegetable food. Although he denied it, yet I was told in the ward that he had upon some occasions partaken of potatoes or some bread with a meal. However, not only was the quantity of urine and sugar diminished by his treatment to so marked an extent, but he improved in appearance, gained in strength, and lost his insatiable thirst and voracious appetite. When first admitted he scarcely left his bedside at all, but soon after his change of diet, he grew cheerful and moved actively and freely about.

Besides attention to diet, the various medicines mentioned in the table were given, to see if any direct effect could be produced beyond what was attributable to the food. But, as in the former case, I am not prepared to say that I am enabled to speak of any unmistakeable positive influence as having been produced. Notwithstanding the large amounts of the acetate of potash and the Rochelle salt that were at times given, the urine was never found otherwise than decidedly acid.

CASE III.

In the following case the sugar underwent a complete disappearance from the urine, after restriction to animal food, had been for a short time observed.

J. B—, æt. 57, admitted into John Ward, under my care, the commencement of September, 1861. His complaint set in, about Christmas last, and he at one period passed as much as six quarts of urine a day. He had formerly suffered rather severely from the usual symptoms of diabetes, but not quite so much so of late. He has no affection of his lungs, and has nothing the matter with him in any other respect. He came to me from the country, and I placed him for the first few days upon the full mixed diet of the hospital, which is about such as he had been recently accustomed to. Having obtained a starting-point for comparison, I then restricted him for a while to animal food with greens. The result is shown in the table that follows:

Date.	Quantity of urine in oz.	Sp. gr. of urine.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar per 24 hours in grs.	Diet and medicine.
Sept. 2nd...	85	1039	44·40	3774	Full mixed diet; sherry ℥vj. Pulv. Opii gr. j ter die.
„ 3rd ...	94	1040	44·40	4173	
„ 5th ...	82	1039	46·15	3784	
„ 6th ...	50	1036	28·90	1445	Animal diet, consisting of 1 chop, 1 ℔ of dressed meat, and 3 pints of beef tea; with greens for dinner. Brandy ℥v. Pulv. Opii gr. j ter die. Jul. Rhei co., Jul. Ammoniæ, āā ℥ss ter die.
„ 7th ...	66	1018	3·33	219	
„ 8th ...	94	1018	4·13	388	
„ 9th ...	86	1020	Trace of sugar only		
„ 10th ...	54¼	1022	Mere trace of sugar only		
„ 11th ...	51	1019	No trace of sugar		
„ 12th ...	63	1010	No trace of sugar		
„ 13th ...	63	1015	No trace of sugar		
„ 14th ...	108	1009	No trace of sugar		
„ 15th ...	69	1015	No trace of sugar		
„ 16th ...	60	1017	No trace of sugar		
„ 18th ...	92	1012	No trace of sugar		
„ 19th ...	54	1019	2·18	117	Same diet as above, with the addition of 2 ounces of bread. Medicine as before.
„ 20th ...	74	1016	2·66	196	

Date.	Quantity of urine in oz.	Sp. gr. of urine.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar per 24 hours in grs.	Diet and medicine.
Sept. 21st ...	91	1017	2·35	213	The bread omitted. Diet and medicine otherwise as before.
" 22nd...	100	1023	5·71	571	
" 23rd ...	61	1020	3·87	236	
" 24th ...	57½	1022	1·60	92	
" 25th ...	86	1014	No trace of sugar		
" 26th ...	72	1013	No trace of sugar		
" 27th ...	88½	1017	No trace of sugar		
" 28th ...	87½	1014	No trace of sugar		
" 29th ...	84	1014	No trace of sugar		Same diet, with the addition of 2 oz. of bran biscuit.
" 30th ...	72	1015	No trace of sugar		
Oct. 1st.	72	1017	No trace of sugar		Same diet, without the bran biscuit.

In the table above there are two days omitted, the 4th and 17th, from the list. This was caused by the urine having been inadvertently thrown away by the attendants in the ward. The diet was changed after the 5th; and, after a rapid fall in the amount of sugar, it ceased to be present at all on the 11th. There was not even a trace of reaction discoverable by the copper solution. The urine continued in this state till the 18th, and doubtless would have remained so, had a change not been made in the diet. On the following two days, however, two ounces of ordinary bread *per diem* were given with the former food, to ascertain if any recognisable effect would be produced. A reappearance of sugar immediately took place, and although the bread was no longer taken, yet the sugar continued increasing in amount for a couple of days more, before it began to show a descent. A similar quantity of bran biscuit afterwards consumed did not give rise to any elimination of sugar with the urine.

The patient left the hospital, with his urine quite deprived of saccharine reaction, and saying that, he did not then experience anything the matter with him. He had no unusual hunger or thirst, and would not have known, by his bodily feelings, that he was labouring under any complaint. He did not himself perceive any ill effect from what quantity of bread he had taken.

About six weeks after his leaving the hospital, I received a

note from him, from many miles distant in the country, saying, "Agreeable to your request I have sent two days' urine, and am happy to inform you I still continue better." Both specimens of urine had thrown down a copious deposit of lithates; and, on analysis, one was found to contain 7·20, the other 7·32, grains of sugar to the ounce.

CASE IV.

James T. T—, æt. 32, affected with diabetes for sixteen months. At one time he passed eighteen pints of urine a day. He was admitted into Guy's Hospital in April, 1861, under the care of my colleague, Dr. Gull, who treated him by restriction to an animal regimen. On his admission, he passed eight pints of urine a day, having a sp. gr. of 1045. His urine underwent a marked diminution in quantity, and improvement in quality. He gained in weight to the extent of ten pounds between April and September, and became stronger, and felt well as regards his bodily sensations. At first, he suffered from the privation of vegetable food, the meat not being of sufficient bulk to satisfy his hunger. Soon, however, his appetite diminished, and he became so accustomed to his animal food, that according to his account, he had not the slightest desire for a diet of any other kind. Having fallen temporarily under my care, he was afterwards, through the courtesy of Dr. Gull, transferred altogether. The following are the results that I obtained from the examination I conducted of his case. The urine was collected from between ten o'clock one morning and ten o'clock the next.

Date.	Quantity of urine in oz.	Sp. gr. of urine.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar per 24 hours in grs.	Diet, medicine, and remarks.
Sept. 1st ...	60	1036	17·10	1026	Cooked meat, 1½ ℔; 1 mutton chop; 2 eggs; 2 pints of jelly (made without sugar); greens; brandy ℥vj; 2 bottles of soda water. Mist. Hæmatoxyli co. ℥j ter die. Pulv. Opii gr. j nocte mane que. Was suffering frequently from relaxed bowels.
„ 11th ...	60	1033	6·0	360	
„ 20th ...	56	1037	12·0	672	
„ 22nd...	58	1034	14·11	818	
„ 23rd...	58	1036	15·	870	
„ 30th ...	56	1035	18·69	1046	
Oct. 1st ...	62	1033	18·21	1129	
„ 2nd...	38½	1034	12·84	494	
„ 3rd ...	41	1034	11·40	467	
„ 4th ...	39½	1027	10·26	405	
„ 5th ...	39	1031	11·40	444	
„ 6th ...	41	1032	12·63	517	
„ 7th ...	42	1033	11·88	498	
„ 8th ...	40	1033	12 63	505	
„ 9th ...	37½	1036	11·79	442	

Date.	Quantity of urine in oz.	Sp. gr. of urine.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar per 24 hours in grs.	Diet, medicine, and remarks.
Oct. 10th ...	34	1031	9·45	321	Cooked meat, 1½ lb; 1 mutton chop; 2 eggs; 2 pints of jelly (made without sugar); greens; brandy ℥vj; 2 bottles of soda water. Mist. Hæmatoxyli co. ℥j ter die. Pulv. Opii gr. j nocte maneque.
„ 11th ...	36	1030	7·50	270	
„ 12th ...	48	1031	3·99	191	
„ 13th ...	35½	1031	8·76	310	
„ 14th ...	36	1032	8·07	290	
„ 15th ...	37½	1030	4·70	176	
„ 16th ...	33	1031	3·52	116	
„ 17th ...	45	1031	7·74	348	
„ 18th ...	46½	1031	8·	372	
„ 19th ...	42	1032	10·42	479	
„ 20th ...	50	1031	8·42	421	
„ 21st ...	47	1031	8·56	479	Same diet. Mist. Hæmatoxyli co. ℥j ter die. Pulv. Cretæ co. c. Opio ʒss ter die. Diarrhœa had set in.
„ 22nd...	39	1031	2·96	115	
„ 23rd ...	38	1030	6·48	246	
„ 24th ...	34	1031	4·52	153	Same diet. Plumbi Acet. gr. j, Pulv. Opii gr. j, in formâ pilulæ ter die. Was suffering from severe diarrhœa.
„ 25th ...	48	1032	11·16	535	
„ 26th ..	41	1034	12·96	531	
„ 27th ...	40	1037	14·10	564	
„ 28th ...	43	1036	16·	688	Two pairs of small soles; cooked meat, 1 lb; 2 pints of jelly (without sugar); port wine, ℥viij; Dec. Krameriæ co. ℥j 6tis horis. Pulv. Opii gr. j nocte maneque. Diarrhœa abating.
„ 29th ...	42	1038	17·14	719	
„ 30th ...	61½	1037	20·55	1263	
„ 31st....	75½	1036	20·55	1551	
Nov. 1st ...	63½	1039	21·15	1343	
„ 2nd...	82	1036	18·	1476	
„ 3rd ...	72	1037	19·44	1399	Same diet. Acidi Hydrocyan. dil. ♏iij, Tr. Calumbæ ʒj, ex Mist. Effervesc. ℥j 6tis horis. This prescription was ordered for sickness.
„ 4th ...	67	1036	16·74	1121	
„ 5th ...	81½	1033	16·74	1364	Same diet, with Brandy ℥vj and 2 bottles of soda water instead of the wine. Tr. Opii ♏x ex Jul. Ammoniæ ℥j 4tis horis.
„ 6th ...	58	1031	15·	870	
„ 7th ...	65½	1033	15·63	1023	
„ 8th ...	71½	1030	15·63	1117	
„ 9th ...	95	1034	15·63	1484	
„ 10th ...	75½	1034	15·30	1155	
„ 11th ...	87½	1031	15·63	1367	
„ 12th ...	81	1036	15·63	1266	
„ 13th ...	64½	1036	16·35	1054	
„ 14th ...	66	1035	16.35	1079	
„ 15th ...	87½	1036	18·93	1656	
„ 16th ...	91	1036	18·93	1722	Same diet, with the addition of greens; and, bacon to be substituted three times a week for the meat. Potassæ Citratis ℈j, Tr. Opii ♏x, Mist. Camph. ℥j, 4tis horis.
„ 17th ...	76	1037	19·98	1518	
„ 18th ...	60	1036	19·44	1166	
„ 19th ...	83	1036	19·98	1658	
„ 20th ...	74	1036	16·74	1238	
„ 21st ...	72	1035	18·45	1328	
„ 22nd...	92	1036	21·15	1945	
„ 23rd ...	88	1036	15·30	1346	
„ 24th ...	86	1039	18·45	1586	

Date.	Quantity of urine in oz.	Sp. gr. of urine.	Quantity of sugar per oz. of urine in grs.	Quantity of sugar per 24 hours in grs.	Diet, medicine, and remarks.
Nov. 25th ...	72	1036	17·13	1233	Same diet. Mist. Hæmatoxyli co. ℥j 4tis horis. Pulv. Opii gr. j nocte maneque.
„ 26th ...	86	1035	17·13	1473	
„ 27th ...	82	1034	18·	1476	
„ 28th ...	79	1036	18·45	1457	
„ 29th ...	72	1037	19·44	1399	
„ 30th ...	71	1034	15·	1065	
Dec. 1st ...	76½	1036	18·93	1448	
„ 2nd...	87	1035	18·45	1605	
„ 3rd ...	58½	1037	15·30	895	
„ 4th ...	81½	1035	15·30	1246	
„ 5th ...	96½	1034	17·55	1693	
„ 6th ...	74½	1036	17·55	1307	
„ 7th ...	66	1037	19·44	1283	
„ 8th ...	76½	1036	17·13	1310	
„ 9th ...	63	1037	16·35	1030	
„ 10th ...	87½	1033	16·35	1430	
„ 11th ...	99½	1033	16·74	1665	
„ 12th ...	99½	1031	18·93	1883	
„ 13th ...	47½	1036	15·	712	
„ 14th ...	46	1033	11·79	542	
„ 15th ...	96	1032	15·	1440	
„ 16th ...	78	1036	16 74	1305	
„ 17th ...	61½	1037	15·63	961	
„ 18th ...	78½	1034	15·30	1201	
„ 19th ...	63½	1035	15·99	1015	
„ 20th ...	68	1035	17·13	1164	

The disease in this patient had thus at one time, taken so favorable a turn, that the sugar eliminated only amounted to 116 grains one day and 115 grains a few days later. He was for a long time presenting the appearance of robust health; and, feeling so strong and well as he did, he was sanguine of making a complete recovery. He had, for as many as nine or ten years back, been subject about three times a year to violent attacks of diarrhœa, which so prostrated him at the time, that he could scarcely walk up stairs. Since he had been affected with his complaint, and whilst on a mixed diet, before admission into the hospital, he suffered from an attack of diarrhœa which lasted four or five weeks and made him so weak that he was hardly able to leave his bed. The attack of purging, and afterwards sickness, that occurred during the latter part of October and the commencement of November, reduced him considerably in strength, and increased the elimination of sugar. Although the sugar afterwards kept up

yet he improved in health, and left the hospital a few days before Christmas, feeling sufficiently strong and well, as he thought, to return to his old employment.

A fortnight only had elapsed, when he again presented himself at the hospital and sought admission under my care. He had been seized with an attack of acute lichen, and his appearance had become remarkably changed. He had been obliged to keep his bed, and was so feeble that he could hardly stand. He has ever since been under my care. His skin affection has disappeared, but he has never regained flesh as before. His condition fluctuates, but there is a feebleness of muscular power which is exceedingly striking. He has kept, with only slight deviations, to an animal regimen, preferring it to any other; and the state of the urine has continued about as it was, during the latter period of his stay, when he was in the hospital before.

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